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The principal aims of this study were to identify, select, analyze, and describe educational programs for culturally disadvantaged children from preschool through grade 12 which had yielded measured benefits of cognitive achievement. Identification and selection were accomplished through a literature search and the advice of a panel of experts. Site visits to 98 programs in 31 urban areas assisted in further selection and provided data for analysis. Twenty-one programs were designated as exemplary; in each, pupils had achieved statistically significantly better scores on standardized tests than had controls, or than national normative figures. In Part I of the Final Report the study is described and a tentative analysis of the programs' components is presented. Conclusions include guidelines for program and design and evaluation and possible approaches to cost-effectiveness analysis for compensatory education programs. Extensive bibliographies are also included. (For Part II of this report see UD 007619) (Author)

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FINAL REPORT
Project No. 089013
Contract No. OEC-0-8-089013-3515 (010)

A STUDY OF SELECTED EXEMPLARY PROGRAMS FOR THE EDUCATION OF DISADVANTAGED CHILDREN

PART I
September 1968

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A STUDY OF SELECTED EXEMPLARY PROGRAMS FOR THE EDUCATION OF DISADVANTAGED CHILDREN

Part I

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Palo Alto, California

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U.S. DEPARTMENT OF
HEALTH, EDUCATION, AND WELFARE

Office of Education
Office of Program Planning and Evaluation

UD 007 618

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In our literature search we received much assistance from Dr. William Paisley, Deputy Director of the Educational Resources Information Center for Educational Media and Technology at Stanford University, and his staff.

This study was the work of a team which included at various times William Abbott, Peggie Campeau, Ettore Coluzzi, Anne Klein, Debbra Michaels, Caroline Smiley, Kasten Tallmadge, and Thomas Tanner, as well as the three authors.

David G. Hawkridge
Project Director

SUMMARY

The aims of this study were to identify, select, analyze, and describe educational programs for culturally disadvantaged children from preschool through grade 12 which had yielded measured benefits of cognitive achievement.

The written reports of over 1,000 compensatory educational programs were perused in a literature search carried out mainly through Educational Resources Information Centers (ERIC), libraries, and some 300 mail requests. With the assistance of a panel of national experts, a list was compiled of about 100 programs, selected not on the basis of geographical area, grade level, or type of treatment, but because each was believed to have enabled its pupils to make greater gains in measured cognitive achievement than they normally would have made had they not received the program. The list was restricted also by considering only programs reported between 1963 and 1968.

A schedule of site visits was established and carried out, to include eventually 98 programs in 31 urban areas and 16 states. During the site visits, structured interviews were held to obtain all data necessary to decide whether the program under study had indeed provided measured benefits of cognitive achievement, and if it had, to compile a complete description and to conduct an analysis of the components of the successful programs.

The site visits were conducted by five senior staff of the American Institutes for Research, with assistance from other staff on two occasions. The routine for the visits was evolved through a careful pattern of training and adaptation, and normally involved discussions with one or more senior persons in each program, as well as some of their subordinates. Visits were followed up by mail and telephone where necessary.

Site data were analyzed in the Palo Alto offices of the American Institutes for Research, and final decisions were made about the inclusion of each program visited in the set of descriptions which forms Part II of the Final Report. No study was accepted for description unless data available indicated that pupils in the program had achieved statistically significantly better scores on standardized tests than had controls, or than national normative figures.

In the analysis of site data it became evident that few if any compensatory education programs are free from blemishes of sampling, design, testing, data recording, or interpretation. Many apparently successful programs could not meet the strict criteria established for this study. Some that did may have done so through the undetected biases in their data, rather than by their educational significance or success. Inclusion of a program description in this report does not guarantee that its results are better than some not described, whether visited or not.

Part II of this Final Report comprises the descriptions of 21 programs which met the study's criteria. Each description was written according to a specially developed format, to provide a concise yet readable account of the nature, operation, and results of each program for principals, superintendents, and other interested persons. The U.S. Office of Education, sponsors of the study, requested that the descriptions be written so that enough detail was offered for a preliminary decision to be made in a school district about the desirability of attempting a locally modified replication. The descriptions include seven referring to preschool programs; 14, to elementary pupils, and six at the high school level. They range from small-scale experiments for less than 100 children to major programs involving many thousands. Some operate during school hours, others after school. Most are inner-city projects for Negroes or Spanish-speaking Americans, but some serve Appalachian white and other minority groups. The preschool programs claim improvements in intelligence ratings, generally speaking, while the others show benefits in terms of reading grade-equivalents. Each is unique in the treatment it provided.

Part I of this Final Report, although much more technical, is an intrinsic part of the study and should not be neglected. Apart from providing background to the study, and an account of its limits, it contains full details of the methods or procedures followed and of problems encountered. The typical characteristics of the programs described are discussed, followed by a tentative analysis of the programs' components. The question of apparently well designed programs which yield unimportant or no measured benefits of cognitive achievement is explored. Guidelines for program design and evaluation are proposed. A closing section deals with possible approaches to cost-effectiveness analysis for compensatory education programs. A list of references is followed by two extensive bibliographies, one providing general sources on the culturally disadvantaged, the other citing all materials collected during the study and relating to programs not described.

In the tentative analysis of programs described, several common components were identified, such as a pupil-to-adult ratio of no more than seven to one. It is possible that these are components critical to the success of programs in yielding measured benefits of cognitive achievement, but no firm conclusions could be drawn on this point, as the study did not include a comparison between common components of (successful) programs described and ones (not successful) studied but not described.

The guidelines offered for program design and evaluation concentrate upon a detailed, comprehensive, and accurate approach using appropriate statistical tools.

The discussion of cost-effectiveness concludes that there needs to be a greater number of successful programs to choose among, as well as more comprehensive data on inputs and outputs.

INTRODUCTION

Background

Educational programs for children in poverty areas have become part of the fabric of modern America. They represent a determined and widespread series of attempts by political leaders, educators, and others to offset the shortcomings of the environments in which the poor of America live, by providing additional educational facilities. These facilities have been of many kinds, from summer school programs to preschool programs, from new libraries to field trips.

Twenty years ago probably few, besides political representatives from depressed areas and educators serving them, realized the importance to the nation's life of these programs. Today, economists, political scientists, psychologists, physicians, businessmen, industrialists, and others agree that a heavy burden is carried by American society as a whole while such disparity remains between the results of educating the poor minority. This general concern has been reflected in the legislatures, where bills have been passed which provide federal and state funds for programs designed to improve the educational standing of children who come from culturally different, poverty-stricken districts.

Negroes, perhaps by virtue of their presence in inner-city environments, have been the subjects and beneficiaries of more educational programs than have migrant workers, poor families of Spanish-speaking background, American Indians, and Southern rural or mountain whites. But from all these groups, children come to school disadvantaged to the extent that their culture has not prepared them for school because it has not provided the experiences normal for the children the schools have been accustomed to teaching.

When such children enter school, the cultural barrier between them and the teacher must be broken down. There have been several ways in which this has been attempted. Some programs have set out to train the teacher better (Jablonsky, 1966). The curriculum for such training courses has included the study of behavioral repertoires of children from Negro, Puerto Rican or Mexican cultures, in order to establish learning needs and strategies for preschool children. Examples of such studies are the Early Childhood Project (Deutsch, in New York City and the Tucson Cooperative Project (Henderson and Wetzel, 1968). Teachers have also studied the few available sets of materials developed specifically for use by particular groups of disadvantaged children.

Other programs have sought to provide early training for the pupil, so that he will understand better the new cultural milieu of the school. The Perry Preschool Project is an early example of this type (Weikart, Kamii and Radin, 1964). Still other programs have tried to improve the culturally different child's chances of learning in the school by providing supporting services during an extended school day. The extra time is used for remedial work, particularly in reading which is one of the chief tools of cultural transmission. The Homework Helper Program is an example (Deering, 1967; Cloward, 1967).

The vast majority of programs, however, seek to hasten the acculturation process, often without due regard for the values of the cultures from which the pupils are drawn, and sometimes without sufficient attention being paid to the exact benefits of cognitive achievement to be derived. The result may be a sense of anomie among those so hurriedly "detribalized," of else profound dissatisfaction among those who find that their acculturation is insufficient to guarantee good opportunities in Western society. Programs which succeed both politically and educationally must be those which are based in the culture of the people they serve and which have high goals of cognitive achievement.

Nobody would pretend that it is not difficult to develop programs with such double goals. This study sets out to establish examples of programs which have at least served the second goal, that of cognitive achievement.

Many of the programs of the past ten years have now been documented, some in books (for example, Bloom, Davis, and Hess, 1965; Hess and Bear, 1967; Gordon and Wilkerson, 1966), others in proceedings of conferences (see Kvaraceus and others, 1965; U.S. Office of Education, 1963), still others in journal papers (Wilkerson, 1965). A survey of this literature reveals that much of it is scarcely useful at all for evaluation purposes. Probably on account of the emotional undertones connected with poverty-stricken minorities, especially the once-enslaved Negro group, many of the writings cannot be judged to be objective; instead, they tend towards the diatribe or the proselytizing address. The better ones are heavily descriptive. Only the best report research results which are statistically sound and which might be used as a basis for future programs.

Unfortunately, it seems true that evaluation of programs funded by local school districts or by federal or state legislation has not been given high priority until very recently. In the midst of responding to the urge to do something about the problems which were painfully apparent, educational research administrators and others perhaps assumed that the success or failure of the programs would be self-evident. Bertrand Russell has claimed that educational research is always successful; while we may disagree with him, there have always been good reasons to be quoted for not attempting too much evaluation. "We have been dealing with humans," we have said, "and they bring too many variables to the research." The system of reinforcement which came into operation as soon as sizeable funds became available from government and other sources was one which had not depended upon quantitative or concise evaluation nor upon measured evidence of vital changes in learners' behavior. It has been sufficient in many areas to prepare reports showing that pupils, teachers, principals, superintendents, parents and distinguished visitors liked the program, believed it to be successful, and thought it should be replicated. The reports in turn have been used by more senior administrators as support for their requests for further funding, regardless of the fact that the reports could seldom show concrete evidence that the most important parts of the acculturation process had indeed been accelerated. Only such acceleration will enable the pupil to shed his disadvantage and take hold of his opportunities in our industrialized society, where gross cultural differences are not easily tolerated. Because political pressures have ensured further supplies of money, many administrators have succeeded in their requests for extensions of their programs. Thus not only their fund-seeking behavior has been reinforced, but also the inadequate procedures for evaluating the effectiveness of the programs themselves. Further abetting this process has been the scheduling deadlines for funding opperations which, in many cases, have necessitated that proposals for program renewal be submitted prior to the availability of data on original program success.

The educational world in general has tended to support the system too. The socializing purposes of American education, of vital importance in this century, have been harnessed to the newly apparent task of dealing with the problems of disadvantaged children. Many educators have claimed that the solution to these problems lies in socialization, in persuading members of poverty groups to become sound citizens and pursuers of the common good. Consequently, affective objectives feature prominently in the programs. The programs' success in these areas is usually measured by relatively unsophisticated questionnaires to faculty and students; in the questionnaires the questioner's attitude and role of authority is often only too clear, and a positively skewed distribution of responses is ubiquitous and practically inevitable. "Good" programs in the nation are identified by many in the educational world as those in which some hypothetical happiness index has been raised, however temporarily, and the impact of reality has been postponed by introducing an atmosphere of sweetness and light into the classroom, if not into the ghetto.

Sometimes confused with those who seek bland euphoria for disadvantaged children are those who seek motivation. The confusion is not surprising, since the methods suggested are often strikingly similar. "Motivation" is the touchstone for success. If pupils can be persuaded to want to work in school, all will be well. Nobody would suggest that motivation is not vital, but the work set for the motivated pupil is equally vital. If he is motivated only to become verbally fluent (in whatever dialect) or to "participate" in class, the most vital parts of the acculturation process are being neglected, at immeasurable cost in the future to the pupil and to our society. Unless motivation for specific task accomplishment, e.g., learning to read, to solve problems, to study art, to develop scientific thinking, can be enhanced, there is no assurance that the disadvantaged child will be more able to succeed once he is beyond the protective pale of the school.

Concurrent with the recent demand for new programs yielding benefits of cognitive achievement, there has been a growing determination to attempt to devise analyses of existing programs. Planners would like to identify the components of successful programs, then proceed to predictions of success and its cost and eventually to the design of new programs containing components with the most favorable cost-effectiveness ratios. Since 1960, and especially since the President's announcements of August 1965, the spread of Planning-Programming-Budgeting Systems (PPBS) and related methods of analysis has brought a new approach to much of public accounting in the United States. Hitch and McKean's (1960) classic, The Economics of Defense in the Nuclear Age indicated the scope of such techniques in defense. Williams' monograph (1966) discusses their application to university planning, while Hatry and Cotton (1967) encourage PPBS at the state and local level.

The realization that such analysis may after all be feasible has come at the same time as certain crises in America's history. While America remains the strongest nation economically on earth, her resources are the subject of competition by rival interest groups. There have been civil disturbances at home, where the social conflicts are increasing. This, and the exercise of a very strong political and military influence abroad, have caused interest groups to question closely the expenditures of federal money by their rivals. The defense "lobby" thus questions economic opportunity expenditure or economic aid to non-aligned developing countries.

Education, including the education of the disadvantaged, has not escaped the general demand that expenditures be justified by hard evidence rather than opinion. The Office of Education has shown itself to be fully aware of this demand, and statements by top officials over the past year or more have particularly stressed the need for evaluation of programs.

The techniques of cost-benefit analysis and cost-effectiveness analysis have seldom been applied in education, although these and other techniques were discussed at a recent Washington symposium, entitled Operations Analysis in Education. Dr. Gerhard Colm, a member of the Advisory Group to the State-Level Finances Project of the George Washington University, has pointed out elsewhere that:

> In the military field, cost-effectiveness studies have been most useful when .... a single clearly defined objective .... could be pursued by alternative methods. In many civilian

> programs (including education), we cannot assume that one single goal is given - the same program often serves several objectives. ..... techniques for determining optimal solutions for programs with multiple objectives have yet to be developed. The problem is further complicated by the present difficulties in obtaining quantitative measures of many important economic and social impacts of programs, even if they are comceptually clearly defined.
>
> At the present time, no tested methodology is available ....*

The field of cost-effectiveness analysis in education is scarcely explored, as Wildavsky (1966) has noted. One of the first realistic incursions was made in the study of Technomics Inc. of the feasibility of cost-effectiveness analysis for Title I of the Elementary and Secondary Education Act, prepared for the Division of Operations Analysis, National Center for Educational Statistics, U.S. Office of Education (Technomics, 1967). The conclusions of these and other studies are discussed later in this Final Report, but may be summarized here. In short, cost-effectiveness analysis in education is premature until the inputs and outputs of the schools are more easily identifiable and more comprehensively measured.

This study early encountered similar problems as those which led to the conclusion that cost-effectiveness analysis in education is premature, and the decision was taken to pursue instead the objectives of describing and analyzing the components of programs which had yielded benefits of cognitive achievement. The descriptions could serve the purpose of providing examples of programs worthy of replication, with local modifications, while the analysis of the components might provide a basis for the eventual development of realistic and cost-effective models for future programs. To serve these ends, it was necessary to identify and select those programs which met certain strict criteria.

Limits of this Study

These criteria can be explained best by means of a glossary. At the same time the limits set by the U.S. Office of Education should be established, distinguishing them from those set by the American Institutes for Research. The Office of Education asked that the study include educational programs for disadvantaged children. "Educational programs" was interpreted widely to encompass not only those providing formal instruction in schools but also those seeking out children in their homes or on the streets. "Disadvantaged children" was taken to be a generic term, however inaccurate or disliked, which included children of Negroes, Mexican-Americans (both migrant and resident), Puerto Ricans, American Indians, and Southern rural and Appalachian Whites.

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* In his Preface to Hatry and Cotton (1966).

The Office of Education further asked that the study include programs serving children from preschool to grade 12. This was interpreted widely to include the Infant Education Research Project in Washington, D.C., which began tutoring children from the age of 15 months. The final specification of the Office of Education was that each program show "measured benefits of cognitive achievement." The American Institutes for Research, on advice from the Office of Education, interpreted "measured benefits" to mean gains, measured by standardized tests, significantly greater than pupils would have made had they not received the program. Normative progress was judged from control groups or normative data. "Cognitive achievement" was at first taken in its widest sense, with the result that a few foreign language and science programs were included in the early stages. It was later decided to limit the study to programs aiming at increasing cognitive achievement in language or number of both. Standardized tests in these fields were judged by the American Institutes for Research to include intelligence tests, since these were in the opinion of the A.I.R. research staff more measures of achievement than of innate ability in the context of the programs.

The American Institutes for Research decided to include only those studies reported later than 1962, since earlier ones would be too far removed from present-day conditions.

Related Research

It is inappropriate to include under this heading the many documents on which this report is based, as these are referred to in other places in the body of the report itself. Rather, related studies and surveys of compensatory education in the United States should be quoted here, to indicate their relationship to this study.

Compendia of compensatory programs have appeared from time to time, ranging from an early booklet prepared by the Office of Education (U.S. Department of Health, Education, and Welfare, 1963) to volumes such as those of Hess (1965), Gordon and Wilkerson (1966), and Pines (1967). Other surveys have been published as journal articles (see Wilkerson, 1965). While many of these contain descriptions of programs, the descriptions were not prepared as part of a deliberate evaluation, consequently they contain much qualitative and impressionistic material not suited to the purposes of this study, in which quantitative data were the basic criteria for selecting programs for description. The compendia were indeed helpful in establishing initial lists of programs to be investigated, as well as providing secondary sources for the description writers in this study.

Several sources of national information were also examined. The Coleman Report (1966) on equality of educational opportunity helped the A.I.R. research team to see the broad picture of education for disadvantaged children across the nation, as well as pointing out some sophisticated (if sometimes mistaken) statistical techniques for the analysis of data. A report by Peerboom (1967) on pre-Title I compensatory education programs was made available to A.I.R. by the Division of Compensatory Education of the Bureau of Elementary and Secondary Education in the U.S. Office of Education, and highlighted a number of programs for consideration. Also, the National Advisory Council on the Education of Disadvantaged Children has published a series of reports on compensatory education nationally (e.g., National Advisory Council, 1967). The Council provided a complete set of these reports for this study, and general trends were identified from them as background.

The Division of Compensatory Education, referred to above, has undertaken several in-house surveys, the reports of which were also made available to A.I.R. The basis of evaluation used in these surveys was different from that of this study, since the surveys did not attempt in-depth analysis of selected programs. Similarly, other in-house studies made of compensatory education by the Office of Program Planning and Evaluation of the Office of Education were of only indirect relevance, but did provide valuable background.

The single source which provided most direct help during the course of this study was probably the Technomics (1967) report. While the approach and techniques used in the Technomics project could not be transferred in toto to this study, the A.I.R. staff was able to gain much from Technomics experience.

METHODS AND PROCEDURES

Selection of Programs to Visit

Literature search. The initial source of information on programs for educating disadvantaged children was an extensive search of the literature. The primary purpose of this activity was to develop a large pool of educational programs from which subsequent selections could then be made for in-depth study. Information collected during this stage included basic program identification, description of the pupils involved, the behaviors measured and benefits claimed, and the names and addresses of appropriate contact personnel. The form employed for recording this information is included as Appendix A.

In order to facilitate the selection of programs to be visited, a two-way rating system was developed. Programs were classified according to whether they included controls and apparently achieved measured benefits; whether they apparently achieved benefits but were lacking in controls; and, finally, whether the study involved controls but without measured benefits. Secondly, on the basis of a quick review of the project report or its summary, a design rating form was completed (see Appendix B). This form was developed during the study as an instrument for gross evaluation of program quality in terms of whether the report provided information on such essential items as population characteristics, sampling procedures, program objectives, criteria involved, measuring instruments employed, and completeness of the evaluation findings.

The Educational Resources Information Center (ERIC) of the U. S. Office of Education provided an excellent source of information on documents, particularly concerning earlier programs. Probably the single most comprehensive index of documents on educating the disadvantaged is the ERIC report Catalog of Selected Documents on the Disadvantaged (1966). Other USOE compilations of project and proposal information, such as Pacesetters - Innovation in Education (USOE, 1966) and Current Project Information (USOE, 1967) served as valuable additional sources. In addition, the monthly copies of the ERIC journal Research in Education (USOE, 1966-68) were carefully screened for leads on relevant educational programs.

Arrangements were made with the ERIC Clearinghouse on Educational Media and Technology, located at Stanford University, to review the microfiche copies of the reports which had been identified as worthy of further study based on the information contained in the USOE index publications. Two staff members of AIR spent approximately 6 weeks reviewing microfiche and other materials at this ERIC Clearinghouse. Sometimes, it was possible to accept or reject an educational program based upon a review of the abstract; however, in many cases a more complete study of the report was necessary. The criteria established for the selection of educational programs at this stage were as follows:

a) Program report completion date of 1963 or later

b) Directed toward disadvantaged children at the elementary or secondary level (preschool to grade 12)

c) Displayed gains in cognitive achievement, with the primary focus upon reading or language arts, mathematics, or IQ

d) Showed evidence of good experimental design sufficient to justify some degree of confidence in the findings

Bibliographic information was also obtained from a number of other sources. At the request of A.I.R. the Science Information Exchange (SIE) of the Smithsonian Institution prepared a list of references to relevant educational research projects in progress. Also of particular help were the references contained in the bulletins of the ERIC Information Retrieval Center on the Disadvantaged; the references to educational programs contained in Gordon and Wilkerson (1966); and the Hess Inventory of Compensatory Education Projects (1965).

Whenever documents could not be located through the local ERIC Clearinghouse, the facilities of the Stanford University Cubberley and Main Libraries were used. If this failed, copies of relevant documents were ordered by mail. Mail requests were also sent to school districts and other agencies to obtain up-to-date information on programs where the only available information was contained in abstract form, in a proposal document, or in very early progress reports. All told, some 300 such letters were sent out.

It is estimated that approximately 2000 documents were screened during the literature search phase. From this phase a pool of programs was derived, 185 of which were selected because they appeared to meet all or most of the criteria established for inclusion in the present project, as listed previously. For each program selected at this stage, three cards were completed: a bibliography card, a project description card, and the design rating form described earlier in this section. Of these 185 programs, 93 were tentatively designated as worthy of further study while the remaining 92 were considered as reserve or back-up programs.

Conference of experts. Project funds and the time available did not permit site visits to all of the programs that had been identified during the literature search stage. This condition had been anticipated during the early planning of the present project and accordingly preparations had begun, even before the start of the project, to assemble in Palo Alto a panel of nationally recognized experts to provide assistance in final program selection. A two and a half day conference was convened six weeks after the project was initiated. In attendance were 10 national experts, three officials of the U. S. Office of Education, and the A.I.R. staff. A list of consultants attending the conference is presented as Appendix C.

While the conference provided valuable guidance concerning a variety of problem areas, including program categorization, component and benefit identification and categorization, and data analysis, its primary purpose was to assist A.I.R. in the selection of educational programs for in-depth study. As a result of the conference review and discussion of the individual project cards, 80 programs were selected as definitely worthy of in-depth study. The remaining programs were placed on a reserve list.

The resulting list of programs to be visited was then submitted to the U. S. Office of Education, to the Carnegie Corporation, and to the National Advisory Council on the Education of Disadvantaged Children. At the same time, the literature survey was being continued on a reduced basis in order to exploit all available sources of information on new educational programs. Based upon the suggestions received and the new program references identified, detailed preparations for the site visits were begun.

Site Visiting

Site visit preparation. Because of the need to obtain a great deal of specific program information in a short period of time, it was necessary to develop a site visit routine which would ensure that all necessary information was obtained with a minimum burden upon program personnel. It was also deemed essential to avoid unnecessary structuring of the interview and the consequent stifling of promising discussion areas.

As a first step in developing site visit procedures, the Project Director and the Associate Director made preliminary visits to several programs. While a draft checklist had been developed on the basis of past experiences and the results of previous studies (Technomics, 1967), the interviewing at this stage was only minimally structured. The primary objective was to obtain information on the effectiveness of alternate site visiting strategies, leading ultimately to the development of a balanced approach, i.e., neither completely unstructured nor unnecessarily rigid, which could then be applied by all site visitors.

As a result of the experience gained during these visits, the initial site visit checklist was revised. The checklist, in fact, continued to undergo minor modifications throughout most of the data gathering phase. Some 100 separate items of program-related information were solicited under the following headings:

a) Basic Data

b) Neighborhood Characteristics

c) School District Characteristics

d) Pupil Characteristics

e) Personnel Characteristics

f) Treatment Characteristics

g) School (Personnel)

h) School (Provision of Services)

i) Costs

Site visit execution. The site visits were conducted by five senior staff members, with the assistance on two occasions from other staff. In preparation for a specific site visit, all the relevant literature available to A.I.R. was reviewed and compared against the site visit checklist of required information. By this process, it was possible to determine those areas where adequate information was already available and those areas where the site visitor should concentrate on obtaining first hand information. In this way, the time of both the program personnel and of the site visitor could be conserved.

The site visitor was provided with a file containing all available reports on the program, any correspondence with persons in that city, and a marked-up copy of the site visit checklist. This material was studied en route to the program site.

Site visits were scheduled to those locations where information could be obtained on one or more programs selected as definitely worthy of in-depth study. In addition to having background materials on these high priority programs, however, the site visitor was supplied with the available materials on all of the compensatory educational programs that had been identified with that particular location. In this way, he was able to obtain first hand information on those programs that had been tentatively rejected or placed on the reserve list during the literature search phase of the present project. This served as an excellent cross check at very slight additional cost to the project. Also during the interviews with school district evaluators and compensatory education coordinators, the site visitor was able to identify any other eligible programs which had not been identified up to that time.

The site visitors had appointments arranged with the senior persons available for each program, and, depending on the program, with one or more of their subordinates. A particular effort was made to meet first with the head of the evaluation team; next, a meeting was often arranged with the coordinator of specially funded programs. Finally, the program was discussed with those who had administered or actually operated it. In general, the site visitors did not visit the classrooms of programs since in most cases the classes had been dismissed for the summer. This was no handicap to the survey, since the most efficient use of the time available was to consult those who were directing or implementing the program. When new project documents were received during the early part of a site visit, these were examined carefully before detailed questioning of the program director.

During the site visits, information was solicited concerning each of the items marked on the checklist. The site visitors also became adept at following slight clues which led to the discovery of significant facts about the program in operation, or about the local context, which would not normally be reported. They quickly learned to extend their questioning in such cases, and to request interviews with other personnel mentioned.

The site visit follow-up procedure comprised several parts. First, the site visitor usually tape-recorded his full impressions, as well as amplifying his written notes from the interview, at the first opportunity. This was often in the evening at a local hotel. Second, the rough transcription of a site visitor's record, together with the program file and any new printed materials obtained during the site visit, was studied by one of the staff in Palo Alto, and a draft was prepared according to a particular format. If there were gaps in the draft on account of information being missing, questions requesting these data were drafted. The site visitor then became responsible for letter or telephone follow-up to secure the missing details. The site visitor was also responsible for sending appropriate letters of thanks to personnel who had assisted in the survey.

Problems in the Analysis of Data

Components and factors which hindered analysis of their effects.

It may seem superfluous to define in this study our concept of a component, except to the few who are familiar with the controversies sparked off by the introduction of the mathematical-statistical techniques of "factor analysis." Here "component" is not merely analogous to the "factors"; it is a close parallel, and in special circumstances could even become synonymous with it.

It is any unitary system of action, pattern of events, collection of artifacts, or combination of these, which may be introduced into the educational system without variations from one point to another; there may be many elements which can be recognized, but if internal relationships are invariant the complete system is the component; if relationships do change with the occasion, the system at once reduces to a combination of several components. Thus if a library is always staffed, the component is made up of the elements books and staff; but if some collections of books are not supervised or controlled by a librarian, "librarians" and "book collections" are two components.

Even for those who find this explanation necessary, this may sound excessively legalistic and verbose, especially when it is known that this study almost certainly though inadvertently has failed to recognize some "components" as collections of elements, and others as mere elements in a larger but undefined component. Yet it should throw some light on some of the problems which were faced.

Little or nothing hindered identification of intended components; they were for the most part, and almost invariably shaped bricks, neatly labeled, clearly identified, and consciously, deliberately, and methodically used to build the programs from the start; carefully defined concepts included in the designs from the beginning.

Once there, nothing encouraged, or even permitted analysis of their effects, for as is well known, it is possible to find particular values for a number of unknowns only when at least as many equations can be found (all of them different) to link them. In other words variations due to single causes can be identified only in the context of variability in application. If for every class one reduces class size to 25, imports teacher aides, gives remedial training by specialists, provides a library with librarian, organizes excursions, buys slide-, strip-, cine-, and overhead projectors, visits the homes, and appoints a clinical psychologist to the staff, it would require quantitatively unique combinations of the above to be able to determine the part played by the psychologist -- if a mathematical solution were possible. Since this study deals with probabilities and not certainties and must use statistical rather than mathematical solutions, scores if not hundreds of schools would be needed, even without the additional complexities of variations in age, sex, curricula, size of school, ethnic groupings, and socio-economic status.

This deliberate identification and inclusion of components yet with cavalier treatment of the demands of analytic studies may be poor research design -- but it is surely sound educational practice. Compensatory education as seen in this study was an onslaught upon the cumulative effects of ages of poverty, both financial and cultural. An onslaught is a battle, and in battle there is freedom to select targets; but a crucial battlefield would not normally be divided into a Latin Square to be able to test selectively the effects of several new armaments or ammunition! In an analogous situation, who would tolerate, let alone expect, unpredictable experimentation from a medical practitioner? If fault there is, it originates with those who provided the enormous funds to blanket the nation's education departments; which did they think they were buying -- action, or research? Or was there another motive?

Action and research are to some extent incompatible. The first seeks to guarantee a predetermined outcome; axiomatically it spares no effort and is entirely dependent upon the existing store of knowledge and information; time is of the essence. Research, on the other hand, is often slow; and unless it deliberately and selectively restricts the scope of action it may seriously handicap the attempt to add new knowledge to the existing store.

Be that as it may, Oakland and Cincinnati Boards of Education are outstanding examples of institutions which mounted huge programs built around substantial numbers of defined components, with little or no chance of evaluating their independent contributions. Both made imaginative, energetic, and purposeful use of the funds they had been given, budgeting carefully. Both tried to incorporate as much practice adjudged compatible with the present state of pedagogical theory as possible, together with innovations for which a priori cases could be made. Each had made full use of the full range of experts from language specialists to programmers of computers, and had made provision for the only kind of research possible under the circumstances: that of determining whether the combined effect of all their efforts and components had produced measureable if not significant improvements in their pupils. They ran remarkably parallel courses and their findings were similar. These might be said to be at least as important as anything described in this report.

However, the attempt to feed a balanced educational diet was not the only obstacle to the determination of the relative importances of the elements, although it must be obvious that no greater obstacle to piecemeal study can exist than thorough and consistent "homogenization" of the ingredients. There are concomitants.

When the achievement of the goal is what matters it is obviously necessary to depend heavily upon the wisdom and insights of those who are most directly responsible for action, and to allow them the flexibility in selection of schools, of classes, of pupils, of extent of use of components, and, for that matter, even of selection of components or introduction of unique ones. These freedoms are vital, but would throw heavy burdens on a research design which at least recorded such variations; but such recording presupposes measurement, which in turn requires time, finance and personnel, reducing the resources available for the achievement of results. Small wonder that one such program (admittedly not one of the larger ones) had allowed a budget of about $400 for testing! In this same program its director mentioned that one of the most important duties and functions was that of becoming aware of significant differences in interpretations and implementation, so as to be able to coordinate the programs as well as to be able to interpret the results.

There is one other line of argument which must have influenced senior educational policy makers. Given the present climate and sense of urgency, underscored by the very magnitude of the funds which had become available, no one can be blamed for seeking results rather than knowledge, and success before economy. More than that, it is a tenable argument that both research and economy can follow upon success, and economy at least upon failure; if even a shotgun hits nothing why shoot? Otherwise get your bird first and then improve marksmanship. In one way this is even in itself an economy, since any success immediately reduces the target area. This line of reasoning conceals at least two flaws; the analogy of the shotgun breaks down, for pellets do not cancel one another out or otherwise interfere with one another, nor is there such a thing as a failure to bring down a bird because too many pellets hit it. But it is conceivable that one educational component could militate aginst the effects of another, and it is believable that too much of a good thing can be harmful; that excursions can reduce time available for reading; that concentration on reading can handicap the teaching of arithmetic; that film shows can so swamp a child with new facts that he is overwhelmed; that too small a ratio of pupils to teaching staff can result in enough concentrated attention to demands, to produce rebellion; and that transporting backward pupils into a previously successful and fast-moving class in another school, can so slow the pace of the original pupils to the point of tedium, that it more than offsets the gains to the newcomers. No doubt such risks as there were, were well-recognized, guarded against where possible; danger signals were watched for. But maximum gains are always closely associated with maximum risks, and the only reasonable alternative to the rejection of funds is to allow flexibility in their use.

Both Oakland and Cincinnati applied the well known "black box" principle of research; that is to say, given a situation in which processing is too extensive and complex to allow of systematic or logical attention, it is always possible to select points of input and output which do permit observation, measurement, and comparison. Thus for example standardized tests of reading ability with known norms can be applied to groups of pupils fairly homogeneous in terms of age, grade- and, socio-economic status, before the tumult and again after a year of it. Although in practice there are substantial problems involved, in theory comparisons of the before-and-after results, as well as of each with corresponding norms of total populations including the "over-privileged" and "under-handicapped", it should be possible to form an opinion of the overall value of the processing. On the whole it was. Many will have a far higher regard for the mountains of effort than for the mice to which they gave birth. The point is that even if it is accepted that "no price is too high to pay for success, however small", partitioning of insignificant gains is not a practical pursuit. Dr. Jacobs of Cincinnati concludes one of his reports with a summary of the findings and pertinent comment (Jacobs, 1968).

He was dealing with The Elementary Remediation and Enrichment Project, the most extensive Title I effort to improve the education of Cincinnati's disadvantaged children, which began in February 1966, and continued with an even larger scope through the 1966-67 school year. He identifies the following components which among others had been important enrichment activities:

Remedial instruction in fundamental areas
Parent involvement
Provision of learning resource centers
Summer learning camp

Jacobs adds:

> Evaluation of the project in the light of its functional objectives, such as improving self-image and classroom performance,

> yielded few positive results. Occasional small differences were noted favoring project pupils, e.g., classroom marks in reading.
>
> Similarly, findings from the evaluation of special instructional activities generally showed no significant difference. Achievement data comparisons showed only one area of instruction that seemed to produce positive results. Remedial Language Arts instruction seemed to have raised achievement in spelling and language.

Then he reports a common finding associated with little or no objective and measurable cognitive gain -- the subjective approval of all concerned.

> Although significant differences were not found even in the separate evaluation of three project components, assessment of pupil and teacher reaction produced strongly favorable results...

Money might not buy success, but let us at least fail more cheerfully! Dr. Jacobs writes a "postscript" which deserves wide circulation:

> If this project is having a desirable impact on target pupils, as reflected by pupil and project staff judgments, then we must either accept these judgments as intrinsic signs of success or discover new and refined ways of detecting differences where they exist. Perhaps we are looking for change in the wrong places. Perhaps our instruments for measurement are too crude. Perhaps our evaluative techniques are inappropriate. And perhaps no changes are occurring in target pupils. We simply don't know.

It is not coincidence that projects seem to fall naturally into two classes:

1. Very large scale, involving many schools, large numbers of components, very substantial funding, with personnel qualifications covering a wide range and involving a variety of specialists, well able to cover the needs of any research involved, yet with action rather than research as the goal.

2. Small, restricted studies, perhaps one or two components, little or no funds except the dedication of a few persons who are striving to test limited hypotheses or, as often, to vindicate their dedication to a particular school of thought -- people with axes to grind, or at least causes to serve.

Several forces independently reinforce this tendency to a dichotomy. As we have said, research tends to be expensive, and the expenses increase geometrically as the complexity of the design and number of variables increase arithmetically. Complex designs call for enormous samples; society can tolerate research upon itself, most especially in sensitive and vital areas, in small quantities only -- small enough not to cause politically important repercussions, since politics thrives more on action than on wisdom.

On the other hand, innovations occur to individuals not to committees, and one or two at a time. All innovations are suspect until they are proved, and public funds are seldom given for anything that is not a gilt-edged security. The man with the idea must depend upon his own resources; he is likely to be a specialist in one field only, while modern research demands teams of specialists.

Small wonder then that Boards of Education are well equipped to do research, report what research they do critically and objectively, making few mistakes and moderate claims, yet strive rather for results; and that small operators sometimes make enthusiastic claims based upon inadequate designs, often upon erroneous reasoning.

Problems of missing data. In this study it was therefore recognized that the emphasis was on the discovery of new and successful instructional techniques rather than of sophisticated demonstrations of success. In other words, it was hoped that the search would be simplified often by the authors whose descriptions were being read. It was hoped that their orderly and systematic development of their themes, with proper presentation of adequate summaries, data, and tests of the data would make matters simpler. But it was expected that sometimes there would be amateur statistics prepared by professional educators. This would not matter provided that enough evidence could be unearthed to support claims made. Facts could be hidden, but they had to be there. There are few canons and essential steps, and a wide variety of satisfactory designs; where the minimum could not be found explicitly stated, implicit indications were sought.

In one form or another, the following features were essential to the assessment of the program:

Hypothesis -- A description of the new element, or treatment, or method which was hoped to be shown superior to an alternative and of the specific changes it was expected to produce, preferably with a linking rationale.

Population -- Those to whom the treatment could be applied or who could be expected to benefit from it.

Sample -- The smaller, practicable number, chosen from the population for purposes of demonstration, and the way in which it was chosen.

Base -- The reference point of departure or the way in which it was to be obtained; the beacon from which movement could be shown.

Measurement -- Both the instruments used to detect change, and suitable summaries of the values obtained had to be given.

Comparison -- There had to be some way of determining a difference from values derived from the base.

Significance -- It was essential to be able to distinguish reliable or meaningful results from fortuitous ones on a probabilistic footing.

Importance -- A more tendentious concept, but at least in the extreme case it is pointless to quote trivial gains merely because they can be shown to be real.

This study encountered trouble under each of these headings sooner or later. Sometimes a letter or a phone call, or another visit produced clarification, though more often than not information not already obtained was, in fact, unobtainable. Once or twice respondents were nearly irritated by repeated requests. Since there is nothing to be gained by provoking acrimonious debate, the anonymity of the sources will be preserved. Some of the points of criticism could undoubtedly be resolved to the satisfaction of the opposition, if not to the authors of this report. The principles involved, however, are important enough to expand on and to illustrate on occasion. There is no intent here to set up a rigid framework upon which to hang reporting, rather some of the causes for dismay should be displayed.

Most of these causes could be reduced to a single one: The necessity to deduce from inadequate or misleading exposition. This might be put in another way; more errors of omission were encountered than of commission. Each of the essential features will serve to focus the discussion of what was lacking.

Hypothesis. As mentioned elsewhere, description was seldom lacking of the components involved in the treatment to be applied; these might be "appointment of teacher aides" or "reduction in class size." It was not as universal to find a description or rationale of the mechanisms of change, or even of the changes that were expected. More often than not this did not matter because the intentions were obvious; for example, reduction in class size yields more individual attention, and if overall school performance is quoted, that is what it sought to change, and such change would be plausible without the need to dot the i's or cross the t's. No example was found of a face issue of paperback novels linked with gains in arithmetical achievement; what were found were remarkably close parallels in which the investigator almost certainly deceived himself as to the hypothesis he was testing by failing to trace linkages and to examine the contents of the test he used; at the very least such cases encourage others to misconstrue the demonstration.

Population. Only infrequently was the population at which the demonstration was aimed properly circumscribed, and again because this was usually clear enough from the description of the sample used. Too often, however, there were aspects of the sample which deserved a close scouting to see whether they were desirable parts of the intended description of the population. An example of this will be found in the next section.

In some cases, several samples were used, each with unique features which made it difficult to decide just what population was intended, and raised doubts about the representativeness of any one of them. Dissimilarities between experimental and control samples increased the problem.

Sample. The easiest sampling method to defend is the random one. It is also, especially in schools, the most impractical and most expensive; the most logical and practical in an educational setting is cluster sampling in which one draws complete school districts or parts of them, or schools, or at least classes. Strictly speaking, this alters the basis for statistical inference, but in a stable system poses few other threats. However, radical changes are in progress in some schools, in some cases leading to increased heterogeneity or even to bimodal distributions on important clusters of variates. In such cases it becomes of considerable importance to determine whether gains have salient associations and this can be done only when the sample is fully described. For example, pre- and posttests of a class, half of whom have been "bussed in", may show gains which may or may not conceal losses in part of the class; but the findings are applicable only to a population with the same composition as the sample. The effect of an additional constraint may not always be known, but its description and emphasis may help to guard against unjustified extension of the results. In one study, voluntary participation by the pupil was essential; there is no way of telling whether the gains obtained could also be expected from conscripts.

In one case, the description clearly indicated that the treatment had been devised for handicapped children; this then was the population. As no control group was used, the most reasonable base would have been "below-normal progress of less than one grade per year", in the ability under study. Progress in the sample under treatment was well above normal -- as it should have been since they turned out to be a group of "over-privileged" children with a large advantage in mean intelligence. As this was a very minor part of the study, it did no harm.

More serious because uncheckable, were the cases where sample losses (sometimes, worse still, with replacement) occurred between pre- and posttesting, so that part at least of the gains claimed could have been due to systematic biases thus introduced. In such cases, only access to the original data can answer questions raised; in an extreme case, for example, a loss of the bottom ten percent can cause an upward shift of one-fifth of a standard deviation to the mean.

Base. The base, explicit or implied, was a prolific source of trouble. Often there was an attempt to provide a control group; if looseness in procedure afflicted sampling, the same looseness appeared in drawing the control group making it possible to double the effects of biases. Sometimes the method of obtaining the experimental sample virtually guaranteed inappropriate control groups; for example, in one case so large an experimental sample was drawn from one school that the remaining pupils had to be supplemented from another school to make the control group large enough, making the constituency of this, at least to some extent, uncontrolled and problematical.

Matching individuals on important variates to provide the control group is a method which has lost popularity over the years; particularly when matching is done simultaneously on two or three variates it can lead to prodigious losses of available subjects whose data can be put to good use with modern statistical techniques. Nevertheless it can be a knightly suit of armour with hardly a chink. One research team unwittingly exposed such a chink for a thrust when it used IQ as a "variate" for matching purposes; IQ is not a single variate, but a combination of at least two important ones (capacity and age) arranged in such a way that a deficiency in one of them can be compensated for and concealed by a low value in the other, making it theoretically possible to have completely dissimilar experimental and control groups.

A fallacy with wide acceptance, and which marred a significant proportion of studies, some of them large ones with considerable financial backing, is that modern statistical techniques (notably analysis of covariance) can compensate for radical differences between experimental and control groups and even for different tests of the same general description. This is no place to deal with such complex considerations in any detail though some of the problems of, and objections to the procedure will be touched on later. Suffice it to say that some research workers took unwarranted liberties with these techniques and permitted undue latitude in selection of control groups.

Failure to make specific provision for a base was not necessarily an insurmountable handicap as long as the composition of the sample was clear and measurements given were normative; elsewhere will be found a neat graphical approach to the problem of finding suitable grade norms for "disadvantaged" children against which to check progress. However, scores were occasionally given in a form which did not allow of such comparisons, and the mere demonstration of gain could only be tested against a meaningless "null hypothesis" of "no gain."

Measurement. Of all elements in the research process, the one with the greatest and most pervasive influence, the one most subject to misuse, abuse, or simple misinterpretation is probably that of measurement. This is perhaps true in any field, but more so in the behavioral sciences since there, ratio measures are almost completely absent, and the measures that are used are more dependent upon arbitrary and inconstant choices, with less control and more confusion of the dimensions involved. It was rare to find a project in which the measures used did not give rise to some suspicion. Typical of the problems encountered was the "changing of horses in midstream"; the use of one publisher's test for a pretest because it was designed for non-reading beginners, another's for a posttest because it measured reading achievement; or the use of test A at one level followed by test B from the same stable at a higher level. If the aims had been to establish predictive systems, this would have been logical; but not when the goal is to measure gains on a single continuum. The point is that the title of a test is a rough guide only to its content and number of dimensions involved. Not only is "Reading Readiness" not synonymous with "Reading Achievement", but even two reading achievement tests may differ in the influences which can produce change. In such a situation there is a strong temptation to which most succumb, to try to compensate for crudity of experimental designs by using highly refined statistical processes, but that is another story.

Even when a study was neat and tidy, and efficient in its choice and use of tests, often in this study the statistics needed could not be found; or else they were suprisingly selected; or on occasion, gross arithmetical errors were suspected and found, or strong evidence that these must have occurred. Why in this day of desk calculators if not computers, did so many depend upon the less efficient median instead of the mean? And having done so made no attempt to test the significance of the median gains claimed even though the standard error of the median is to be found in elementary textbooks? Worse still (even when means had been calculated sometimes), many did not even give standard deviations; understandably hardly anyone quotes original data, which left for this study only devious expedients and approximations in a few cases, and no hope of a fair assessment for a larger number. In at least one case such gross arithmetical errors had been made in simple subtraction of means that a "significant" gain disappeared when recalculated. In another, significances of gains had been tabulated, with almost no supporting data; it was possible to show that one of the missing items, correlation of pre- and posttests, separated by an interval of a year, must have been of the order of .93 -- a surprisingly high figure.

The most important single statistic (after the measure of central tendancy used, mean or median) is the standard deviation, and this was missing far too often; even more often the standard error of the mean was absent which would have been only a little worrying if the SD had been available. Almost as important, especially when differences between gains for experimental and control groups were being compared, was correlation between pre- and posttests; yet these figures made rare appearances. Reliability estimates of tests used are somewhat more useful than experimenters seem to believe.

Comparisons. It might be thought that if questions of samples, bases, and measurements had been answered, comparisons would be obvious; there were exceptions. Sometimes the lack of careful categorical planning in any other department showed itself here. One study without a control group, and for which normal grade level at the end of a semester should have been the base, found the investigators comparing pre- and posttest scores, and finding them significantly different; a reasonable translation of their findings would have been "the hypothesis that our methods produce no results can be rejected" -- hardly an important contribution.

Comparisons were made, among others, of:

- dissimilar groups;
- pre- and post-tests, with severe attrition in the sample between the two;
- experimental and control groups with the same post-tests but different pre-tests ("adjusted").

Significance. Even when reasonable demands had been met elsewhere, the portions on significance testing were a constant source of frustration; there would be no test of significance when it was obvious that the basis for it existed; there would be medians or means but no standard deviations -- or if by some chance these had been included, the correlations or covariances between pre- and posttests were missing; or the size of the sample would be unknown.

Occasionally all intermediate statistics would be given but there would still be the minor headache of deciding for small samples whether they were statistics (i.e., precise figures for the samples without "correction for bias") or estimates of parameters (i.e., with the $\frac{N}{N-1}$ factor).

Worst of all, there would be significance figures with no supporting data or statement of the test used, or even whether the test was one- or two-tailed. There were even cases where it was known from the reports that F-ratio tests had been used yet it was not certain that they did not refer to the variances of gains which would not mean that the differences between means was significant.

There were even times when it was not clear what the investigators were doing, and there was a suspicion that they did not know either. A simple t-test is most often appropriate, though most authorities believe it should follow upon an F-test. However, when covariance analysis is appropriately done, an F-test is sufficient; but the suitability of covariance analysis is not at all easy to decide on. Certainly it is appropriate when adjustments for reasonably small starting differences need to be made in several groups -- provided the adjustments are not needed because the tests are different. If they are, the whole idea is, to say the least of it, debatable, for then concepts of prediction and regression are brought in where that of equivalence is more appropriate. This will be taken up in a little more detail (though not much, because this is not a textbook, and less negatively). Here it is necessary to emphasize only that a blunt statement of a significance level can be misleading; at the very least the test used should be specified, preferably with a reference; better still, the basic elements should be given to allow one to adopt variations of the test. Above all, interpretation would be considerably eased if investigators always translated the results of these tests into ordinary language.

Importance. Almost everyone, these days, is aware of the risks of using samples that are too small (although research frequently was found based on samples of no more than a dozen or two). Significance tests allow for the sample size, and (apart from the fact that small "samples" that are really "clusters" are likely to be representative only of stringently circumscribed populations) are more likely to be "fail safe" by accepting the null hypothesis falsely.

There is another risk which cannot really be dismissed in the absence of a cost-effectiveness follow-up. The use of huge samples may result, quite correctly in the rejection of a hypothesis of "null gain", when the benefits are negligible. Unfortunately it is far more difficult to evaluate the importance of a gain than to establish its reality. Strictly speaking, one would need to know the cost per head of implementing the discovery, the losses that would continue otherwise, and the costs of alternatives -- an impossible task for the most part. However, in the absence of these refinements, a wet thumb is quite a fair wind indicator; the standardized gain gives quite a good perspective for common sense decisions (i.e., gain divided by standard deviation).

For example, physical height can be used as an easy, meaningful illustration. It is doubtful whether anything which changed average male height by less than half-an-inch would have much of an impact; for a few dollars per head it might be considered, but there are more urgent needs, at thousands per head. Half-an-inch is roughly a fifth of the standard deviation of male height. Yet on control and experimental samples of 5000, one-tenth of an inch difference between the means would be significant at the five percent two-tailed level.

As a rule-of-thumb, gains of less than a third of a standard deviation are nothing remarkable. One needs the standard deviation. Too often it was not to be found, or even to be derived.

Methods of Presenting and Summarizing Data

Considerable attention was paid to methods of presenting and summarizing data collected from reports and site visits. The Office of Education requested that descriptions of the programs selected be written so that enough detail was offered for a preliminary decision to be made in a school district about the desirability of attempting a locally modified replication. Since the data was collected from various sources and in widely differing forms, this request implied the need to reduce the data to a common format in the descriptions.

The description writing guide. Each of the writers of descriptions on the staff of the study used a description writing guide developed during the period of site visits by one of the staff based permanently in Palo Alto. The guide was evolved from a rough draft containing a broad enumeration of those details likely to be of significance to school district personnel. Later versions were based on experience gained from compiling descriptions of three programs subsequently discarded. It was found that most of the descriptions could be divided well into nine main parts:

Introduction
Personnel
Methodology: General
Methodology: Specific Examples
Evaluation
Budget
Quoted Sources
Sources not Quoted
For More Information

Each of these nine parts is explained below.

In the Introduction a quick overview of the program was provided. First, a brief description of the treatment was given, followed by details of the pupils served by the program. The historical development of the program was dealt with next, rather briefly, and the magnitude of the program outlined. Finally, the cognitive behaviors measured were listed together with a very short account of the main results of testing.

The Introduction was intended only to indicate to the reader the salient features of the program, enabling him to judge whether he should read the more detailed description under the other headings.

The personnel involved in a program were listed in categories. After the name of each category (e.g., educational aides), a few notes were usually given in parentheses concerning the qualifications or selection procedures for this category. Were these fairly obvious, as in the case of guidance counselors, secretaries, or school nurses, nothing was mentioned. Then for each category the more important activities and duties were listed, although again nothing was entered for the more obvious cases.

Under this heading there was also noted the time commitment of various categories of personnel, particularly if these people were sharing their time between several programs. Others only tangentially associated with the program being described, such as janitors, cooks, or the district director of research in some cities, were mentioned in a final paragraph rather than by category.

The assumptions and objectives of the program were frequently included under a heading Methodology: General, particularly if they had been clearly laid down by the program director. A narrative outlining the treatment used in the program formed the core of this part, however, including all the major components in as comprehensive a manner as the available information allowed. Not only the instructional methods used with the pupils, but also any training programs for teachers were discussed here. Every attempt was made to provide here a framework of fact about what happened in the classroom, onto which the reader might attach the additional specific examples of the next part of the description.

Where specific examples of principal aspects of a program were available, these were usually included under a heading entitled Methodology: Specific Examples, although some might have been discussed under Methodology: General. The examples were selected from the available publications concerning the program, or in a few cases on the basis of personal observation of the program. They were selected to illustrate the content, methods, or effects of the program; consequently, they ranged from infants' songs to tutors' accounts. Materials found to be particularly useful in the program were listed here or described.

If there was more than one evaluation report available on a program, from two sources for the same year or for several years, the Evaluation part of the description attempted to deal with each, showing both juxtapositions and trends. Hence this part ranged from a few paragraphs for some programs to many pages for others. The evaluation reports were examined critically by the staff of this study, and many programs were not included in this publication because either the reports or on-site inquiries showed that there were no measured benefits of cognitive achievement for one reason or another. Even for the programs described there were warnings inserted for the reader; in some of the descriptions specific comments and caveats were written, as appropriate.

The measures of achievement used were described first, for each area tested, and the test results were summarized (not presented in full) in tables. Wherever possible these were presented simply, although the level of confidence (p value) for the differences (benefits) was cited if known.

Next under this heading other evaluation indices were discussed, usually briefly. The studies undertaken were outlined, and the results summarized, to give a fuller picture of evaluation. Should non-cognitive achievement, such as improvement of the self-image of pupils, have been the major objective of the program, this was mentioned. This portion was not intended to be exhaustive.

The final product of the evaluation(s) was usually modifications and suggestions for changing the program. These were discussed last under the Evaluation heading. Where possible the reasons for the changes were included. From this portion it should be possible to judge what changes might be made (were it adopted elsewhere) to improve the program still further.

The Budget heading was not intended to provide for a detailed account of all the expenditures associated with a program. Such figures were rarely obtainable. Rather it was a description of what components a planner would have to take into consideration in replicating the program, together with whatever rough estimates of per pupil costs or of global costs the researchers were able to secure. It might include typical staffing patterns required, type of materials essential to the program, details of space to be provided in schools, cost-trends over several years, and other relevant items of this nature.

Under Quoted Sources were listed the reports actually quoted in the description.

Under Sources not Quoted other publications or documents known to the researchers which give further details of the program described were listed.

One or more names and addresses were given for people closely associated with the program described or its evaluation. These were correct in August 1968, and listed under the For More Information heading.

The result of the use of the description writing guide was that, although the descriptions were written by several authors, a common format was followed with only minor individual variations. Thus it is possible for a person wishing to know about the populations of a number of programs to turn to roughly the same paragraph, under the same general heading, in each description. Since not as much information is available about some programs as about others, not every feature of the descriptions appears in every description.

Specimen descriptions drawn up according to this format were submitted to the Office of Education, the National Advisory Council for the Education of Disadvantaged Children, and two persons who had been principals until recently, for review and comments. When drafts of the 21 program descriptions included in Part II had been prepared they were mailed to the project directors for correction. Requests for changes, mostly minor, were received from about a dozen directors, and in almost all instances were acceded to.

Graphing the results. A clear graphical summary was sought which would amplify the message carried by tables, both for the technical and the non-technical reader. It was found that those programs in which grade-equivalents had been used to assess status of pupils, in reading or arithmetic, for example, a simple graph could indeed be prepared. This graph would state obviously the need for disadvantaged children to strive towards national norms as a goal, would show the trend of most disadvantaged children away from such norms, and would indicate the benefits provided by a particular program over a given period. Diagram 1 shows the normative achievement of children taking the nation as a whole. The norm may be the subject of controversy, but it is also widely accepted as a pragmatic standard. A child achieving at the norm falls on the line: at grade 3.0 he has grade-equivalent scores of 3.0. Diagram 1 also shows the probable normative achievement of badly disadvantaged American children. Repeatedly in this study data occurred which indicated that on average the badly disadvantaged achieve at about two-thirds of the level of the children at the national norms. Hence the badly disadvantaged child at grade 3.0 has grade equivalent scores of 2.0. Some might argue that the ratio is 7:10 rather than 2:3, but that is of scant importance. The fact is that the degree of disadvantage increases, probably in a fairly constant ratio as shown by the lower line of the graph in Diagram 1.

Having established these two lines as working bases, it is safe to predict that the majority of programs for which results are expressed in grade-equivalents will have results falling between the two lines. What is of interest is whether the individual program is able to induce the line for its pupils to trend towards the national norm. This study examined examples of programs such as that shown by line C on Diagram 2; pupils were actually not learning at all, according to the published results, so were becoming quickly even more disadvantaged.

Other programs yielded results producing line B; in these programs pupils were not becoming any more greatly disadvantaged. Line B parallels the national norm. To arrest the process may seem laudable, but only programs represented by line A could come close to being considered for description. Line A shows pupils gaining fast enough to move towards the national norm, gaining faster than two months in three.

Some of the graphs appear a little more complex because of the number of groups represented on them, but all follow the principles enunciated. A few show pupils below the disadvantaged norm, a very few show achievement above the national norm.

As simple devices to depict measured benefits of achievement, without any test of statistical significance, the graphs serve their purpose.

Diagram 1

NATIONAL AND DISADVANTAGED ACHIEVEMENT NORMS

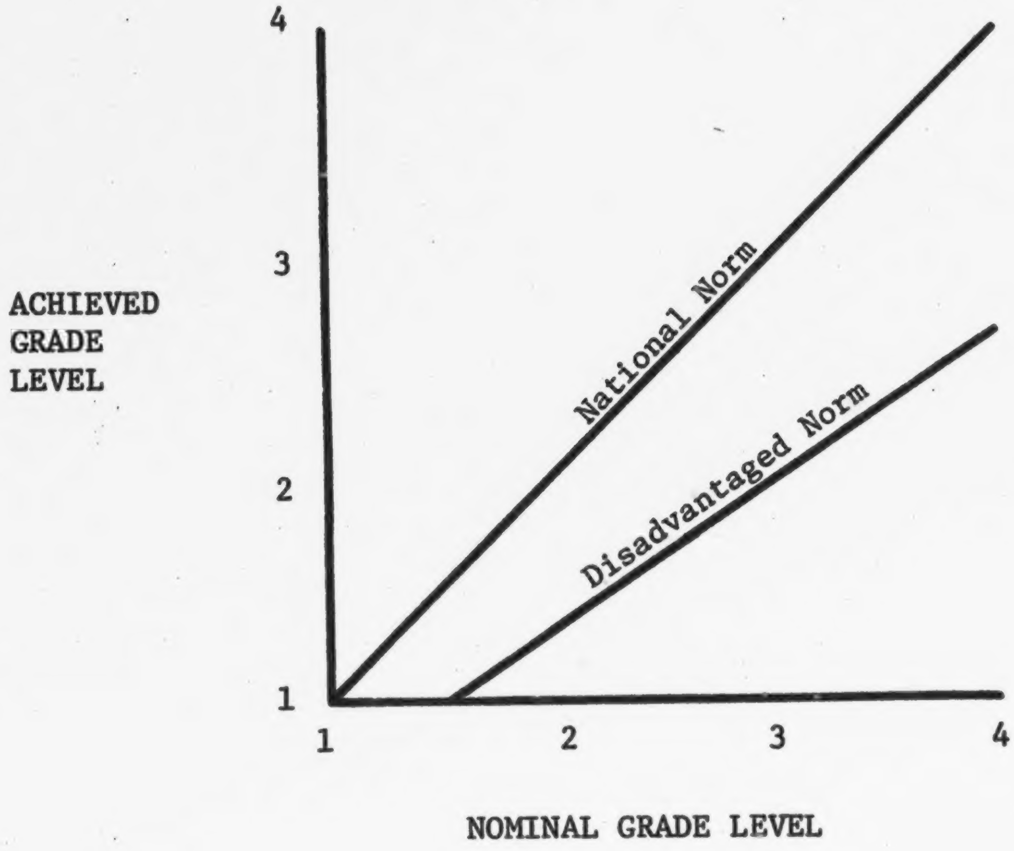
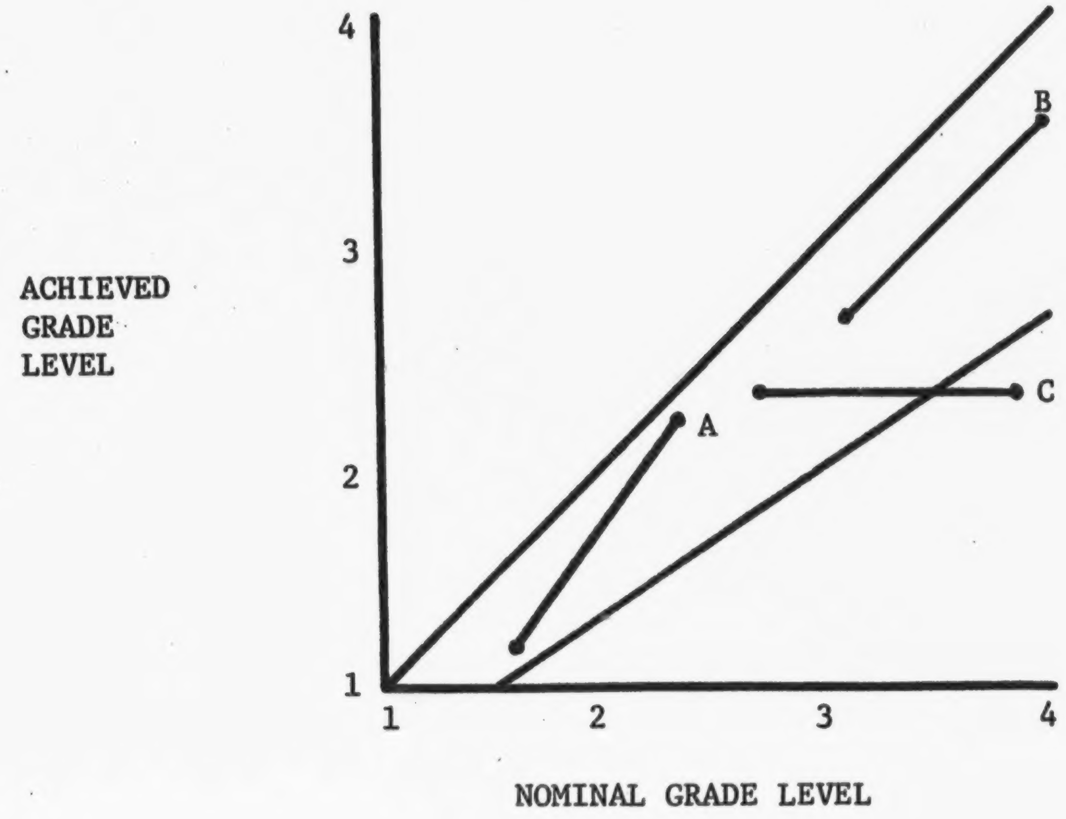


Diagram 2

THREE EXAMPLES OF PROGRAM YIELDS



RESULTS AND FINDINGS

Overview

Altogether, site visits were made to a total of 98 programs, in 31 locations representing 16 states. From these 98 programs, 21 were identified as having met the criteria for acceptance discussed earlier. Detailed descriptions of the programs are contained in Part II of this report. The remaining programs may not have been selected for a variety of reasons, including lack of available data upon which to assess fully the program benefits or failure to demonstrate measured benefits of practical significance. A major problem in the larger cities, e.g., Los Angeles, was the extreme difficulty, or in some cases, the impossibility of untangling the complex net of interwoven programs to such an extent as to permit the tracing of measured benefits to any single program or manageable number of components.

Table 1 presents a complete listing of the programs accepted, their locations, grade levels encompassed, and the number of pupils involved during the latest year that data were available. The 21 programs were located in seven states plus the District of Columbia. Five of the programs were aimed exclusively at preschool children, while two additional programs involved preschool plus other grades. As shown in the table, the fourteen programs involving elementary school children generally spanned four or more grade levels. Only two programs were aimed exclusively at one or two grades. Six of the programs selected for description covered students at the secondary level, grades seven to twelve. Of these, only one spanned the entire range while the remaining five each covered two adjacent secondary grade levels, most often in combination with one or more elementary grades.

From the standpoint of the ethnic composition of the pupils, Negroes were involved in sizeable percentages in 17 of the 21 programs, while children of Spanish surname were similarly represented in 11 of the programs. In only three programs were Appalachian or inner-city whites represented in significant number. In terms of sheer numbers of pupils covered by the programs, the range is extremely wide. Considering only the most recent year for which data were available, the numbers ranged between 15 and 16,600, with a median of 500.

Program Components

Before attempting to summarize the substance of the programs described, it should be mentioned that, while 21 programs were included, actually only 20 programs are prime candidates for large scale replication. One project, Teacher Expectation in South San Francisco, was included primarily to emphasize the importance of teacher expectations for the performance of disadvantaged children and to demonstrate the impact that such expectations can exert upon actual pupil achievement.

Table 1

SUMMARY OF EDUCATIONAL PROGRAMS DESCRIBED

Program Title	Grade Level	Major Ethnic Group	N during Latest Year
The Preschool Programs of Fresno, California	P	S	750
The Infant Education Research Project in Washington, D.C.	P	N	28
The Early Childhood Project of the Institute for Developmental Studies in New York City	P, K-3	N	255
The Perry Preschool Project of Ypsilanti, Michigan	P	N	24
The Diagnostically Based Curriculum in Bloomington, Indiana	P	W	15
The Academic Preschool in Champaign, Illinois	P	N	15
The Homework Helper Program of New York City	3-6,11-12	N,S	2000
The Intensive Reading Instructional Teams of Hartford, Connecticut	3-6	N	500
The After School Study Centers of New York City	2-6	N,S	13000
The Self-Directive Dramatization Project of Joliet, Illinois	1-4	N,S	134
The More Effective Schools Program in New York City	P, K-6	N,S	16600
Project Concern of Hartford, Connecticut	K-5	N,S	260
The Elementary Reading Centers of Milwaukee	4-8	N,W	1000
The School and Home Program of Flint, Michigan	K-6	N	2300

Table 1
(Cont.)

SUMMARY OF EDUCATIONAL PROGRAMS DESCRIBED

Program Title	Grade Level	Major Ethnic Group	N during Latest Year
The Programmed Tutorial Reading Project of Indianapolis, Indiana	1	N,W	1200
Teacher Expectation in South San Francisco	1-6	S	37
The Speech and Language Development Program of Milwaukee	1-2	N,S	273
The Communication Skills Center Project in Detroit	2-12	N	2845
Junior High Summer Institutes in New York City	6, 7-8	N,S	15000
Project R-3 in San Jose, California	8-9	S	74
The College Bound Program of New York City	9-10	N,S	3000

P = Preschool
S = Spanish Surname
N = Negro
W = White

Because this program had no educational components to replicate, the following summary is restricted to the remaining 20 programs.

Preschool programs. Looking first at the treatment duration of the seven programs involving preschool children, we find a near even split, with three programs extending for a single academic year and four extending for two years. In terms of intensity of treatment as measured by the number of instructional hours per week, the programs ranged between 5 and 20 hours per week; however, most were the usual half-day program five days per week. All of the preschool programs emphasized language training and the achievement of a pupil-to-adult (not necessarily accredited teacher) ratio of one to seven or better. The active participation of adults, other than professional teachers, was a typical component of the programs described. These adults were ordinarily parents or aides or both. Field trips were another component present in most of the preschool programs. From the standpoint of benefits expected from the preschool programs, most of the programs described measured program impact in mental ability or IQ. A variety of measures were employed to assess such gains; the two measures used most often were the Peabody Picture Vocabulary Test and the Stanford-Binet, each being used in five of the seven programs.

While the similarities among programs are rather obvious, it would be a mistake to assume that the seven programs involving preschoolers were all of one mold. On the contrary, there were notable differences among the programs both from the standpoint of underlying philosophy as well as in the specific methods employed. In terms of degree of structure alone, the programs ranged from almost total freedom for individual teacher innovation to the very highly structured, where specific teacher behaviors were established to reinforce appropriate pupil responses.

Elementary programs. As mentioned earlier, most of the programs described dealt, in whole or in part, with elementary school children in grades one through six. About half of these could be considered as extending primarily for a single academic year, while most of the remainder were one semester efforts or shorter. Three programs ranged between 5 1/2 weeks and 3 1/2 months. Several of the programs were directed toward providing assistance after school hours, two involved summer sessions, and the remaining programs were aimed primarily at providing special education during normal school hours of the regular academic year. Because of this diversity, it is impossible to provide any meaningful overall summary of the intensity of treatment in terms of instructional hours per week. This diversity extends into the very substance of the elementary programs, making any attempt to summarize program components much more hazardous than for the preschool programs.

All but one of the elementary school programs measured gains in reading achievement. This, of course, was expected since measured benefits in reading was one of the prime criteria for program acceptance. On the other hand, gains in number skills, also a criterion for program acceptance, were assessed in only three of the programs. Despite the predominate emphasis upon reading behaviors, the specific instruments used for assessing such gains varied widely with only two tests being employed in as many as three separate programs.

In line with this emphasis upon reading, the elementary programs often included special reading instruction, using personnel with professional training in this field. Similar to the preschool programs, the improvement of the pupil-adult ratio was a major component; the ratio, however, appeared to range wider, from one-to-one tutorials to one teacher per fifteen students. Also in contrast, the adults involved in the elementary programs were typically professionals rather than aides or parents. Emphasis upon parent involvement was much less apparent than at the preschool level. The component of field trips, so popular at the preschool level, did not appear as a major component in any of the elementary programs described.

Secondary programs. Of the six programs that included secondary school students, two were strictly summer programs, while one functioned both in summer and during the academic year. Of the four programs that did operate during the school year, two provided special instruction approximately three hours per week, while one provided 15 hours of instruction per week. One program was aimed at providing after school assistance using high school students as tutors. All of the programs attempted to assess program benefits in terms of reading gains, while the three programs that were aimed exclusively at junior and senior high school students (grades six through ten) also measured achievement in mathematics. It is interesting to note that students in grades ten to twelve are included in only two of the programs described.

While half of the secondary programs included might be described as remedial reading programs, the remainder emphasize a variety of innovative approaches. In this respect, they appear to resemble preschool programs more than elementary programs, particularly in their employment of aides and the use of field trips.

CONCLUSIONS AND RECOMMENDATIONS

Guidelines for Program Design and Evaluation

Looking back from the vantage point of accumulated experience -- in retroperspective, if the word is permissible -- there are aspects of the programs studied that stand out and call for comment, much of it favorable, and some that prompts positive suggestions with constructive intentions. As always, research designs can take on a variety of shapes even when the same objective is in view. Nevertheless, a firm framework with common features can almost always be found; the superstructure is more varied, yet even there, with no clear line of demarcation, it would seem to be molded to suit some point or range on a continuum from "Proof" research to "Enquiry." That is to say, some patterns suit the purpose of searching for and formulating hypotheses in areas where little is known. Rather different forms are adopted where a substantial body of information already exists, with a network of known and inter-related facts and with selected and fairly clearly defined areas of search. The choice of statistical techniques reflects the objective.

Psychological research, for example often leans towards the "Enquiry" end, and makes a good deal of use of such devices as partial and multiple correlation, or cluster or factor analysis. These were seldom encountered in this investigation. By far the greater proportion of the projects examined were manifestly intended to get plain "Yes or No" answers; there were comparisons between groups, or with population norms using measures of central tendency, with t-tests and occasionally chi-square; even when F-tests were applied it was more often to the results of covariance analysis, embarked on as a refinement which allowed for differences in starting values between experimental and control groups. This will be discussed further later.

For the skeleton of this common theme, names have already been given to some of the more important bones in an attempt to codify difficulties encountered. There is perhaps a slight risk that some may interpret this as a preference for a rigid format along the lines of propositions in ancient Greek geometry; in fact there is more to be gained than lost in such formality as Euclid's. Nevertheless, there is room for individuality as long as essential information is supplied, and supplied explicitly rather than leaving it to readers to unearth, deduce, or interpret. Basic to some reports were several acceptable alternatives, and it is encouraging to have support for this stand from some influential bodies. An example which made for very easy reading comes from the Center for Cognitive Learning of the University of Wisconsin. Their technical reports make use of these headings, although they modify them as needed:

Background -- With some account of personnel involved, accommodation, the population aimed at, related studies and references, special difficulties encountered and the general purpose of the project.

Subjects -- This covered the sample drawn, its chief features and the method of drawing it.

Treatments -- A detailed description was given of the method being tested and of the materials used.

Procedures -- This covered organizational details as well as the provision for comparisons.

Data Collected -- This included short descriptions of the variates as well as their place in relation to specific hypotheses to be tested.

Analysis of Data -- The statistical techniques adopted were given and the data to which they were applied.

Discussion -- Here, a few important tables and charts, but most important, a step too infrequently taken by investigators, a translation into English of the statistical conclusions.

The United States Department of Health, Education, and Welfare used these headings and sub-headings for their table of contents in at least one of their reports:

Summary

- Problem
- Scope of the study
- Objectives pursued
- Hypotheses
- Method
- Results for each hypothesis

Introduction

- Problem
- Objectives

Related Research

Curriculum

Results

- Including data analysis

Discussion

Conclusion and Implications

Procedures

Design
Criteria for selecting subjects
Assessment and diagnostic techniques
Screening
Repeated measures

For one last and slightly different example, we drew from one of the many Center for Urban Education evaluation reports. They used quite a small number of headings and sub-headings, but very efficiently:

Introduction

Description of program
Objectives

Evaluation Procedures

Objectives of evaluation
Selection of schools and classes for evaluation
Data collection
Bases for evaluation
Instruments
Analysis of data
Orientation of examiners and observers

Findings

Conclusions and Recommendations

Within such frameworks as these (and there were several others equally good) it was fairly easy to find one's way directly to each of the focal points. However, this facilitation for readers or users is not the only argument in favor of making the design formal; failure to check against a list of essentials is a source of oversights and blind-spots; worse, it allows loose planning with shifting definitions, side-tracking, and inappropriate offshoots. Good, if personal, checklists are as useful for research design and reporting as they are for traveller's luggage, and some years ago Symonds proposed "A Research Checklist in Educational Psychology" (1956). He intended it for students who were preparing dissertations, but why should qualified investigators be less systematic? Symonds expands on these, his main headings:

A. Scope and Definition of Study
B. Hypotheses
C. Background
D. Definitions
E. Method of Study
F. Design
G. Sampling
H. Studying Personality
I. Tests and Measures
J. Use of Judgment
K. Content Analysis
L. Statistical Handling of Results
M. The (Ed.D.) Report

For those who have not yet discovered it, the "Handbook of Research on Teaching," edited by N. L. Gage (1963) is a miniature encyclopedia (1,200 pages by about 30 authors). It is difficult to pick out the most valuable chapters; most of them contain profit, but particularly relevant here is that by A. A. Lumsdaine, Chapter 12, "Instruments and Media of Instruction," pages 583 to 682; that author gives his purposes as " .... to aid the research worker in sharper, more useful delineation of problems; (2) to analyze the current status of research-based knowledge in the more important problem areas; and (3) to consider methodological techniques and pitfalls in order to improve the incisiveness and efficiency of future research."

There were three more specific areas for which more careful preparation by investigators is recommended:

1. Problems of sampling
2. Study of content of tests used
3. Statistical analyses using
   (i) Medians
   (ii) Analysis of covariance

Problems of sampling. In spite of categorical warnings by statisticians over the years, most of us at some time or other ignore limitations upon the use of tests of significance set by sampling methods, perhaps because most of the authorities put their caveats in much too ponderous mathematical terms. Without the need to justify the objections or to provide corrections, they can be put quite simply:

Significance tests, suitable for those of us whose expertise lies in other professions than Statistics, are derived from theoretical models which depend upon the simplifying assumptions implicit in *random sampling*; and in turn this latter concept is more easily understood than defined. "Randomness" today carries a long list of necessary conditions, the most important of which is that *all individual cases in the population must have an equal chance of being chosen*. This condition is violated when *cluster sampling* (e.g., selection of complete classes, schools, or areas) is adopted and *this* is the obvious method in education. Simple t-tests do not have simple interpretations when this is done.

As by-products, those who feel that they could benefit from another look at the old textbooks can expect to tighten their grasps on concepts of population and statistical inference by reading "Sampling in a Nutshell" by Slomim (1960). This should be followed by the progressively harder publications from Stuart (1962), Yamane (1967), Deming (1950), and Cochran (1963).

Study of content of tests used. It is easy for most investigators to describe the content and essential features of their own aims, materials, and methods; some translate the results of the statistical analysis into more meaningful English, but there is a good deal of room for improvement in the discursive treatment of the content of the measuring instruments used. Failure to do this leads to some deceptive and unwarranted conclusions.

For example, some tests call for elements of interpretations, or subjective rating or assessment; or they are susceptible to subtle influences of sympathy, familiarity, or methods of communication. Certainly statistical treatment will reflect this, but explanations of changes in numbers sometimes reflect wishful thinking, where consideration of test content could have given simpler, if less dramatic explanations. Almost invariably, the sampling process precedes the testing; recent experimentation has shown that this can easily produce startling differences in results via some quite ordinary biasing mechanisms. We not only see what we want to see - we even encourage responses which give superficial but spurious confirmation, and can do this quite unconsciously. Rosenthal and Jacobson (1968) have written a book which ought to be read before the start of any new project. Some of the experiments they describe show what can be done even with animals much lower on the scale than Homo sapiens; and both real and spurious effects can be produced by "expectations," thus recalling the so-called "Hawthorne effect." The work of these two authors will have an impact on experimental design, on treatments, on data collection, and on interpretation of results.

There is another reason for careful scouting and discussion of test content, and that is that where names are insufficient descriptions of content, changes may derive from undescribed and even otherwise unimportant parts, leading to misinterpretation of the results, at the very least. The most frequent example of this is the uncritical use of tests of intelligence to demonstrate gains as a result of some new component, occasionally with a thinly-veiled suggestion that perhaps there are no real genetic differences and that, after all, all men really are created equal.

The criticism is often made that intelligence tests do not measure intelligence, based on the fact that they do not measure it directly; there is nothing unusual about that -- not only the behavioral sciences, but even the so-called exact sciences are often driven to inferred measurement. Physicists have long used the galvanometer to measure electric current by forcing it to generate a magnetic field, and this in turn to produce torque. Or it is said that the tests are suitable for a narrow band of the culture which produced them; again there are parallels from physics, where a mercury thermometer breaks down completely at both very low and somewhat high temperatures. Extraneous influences affect the results of both intelligence measurement and barometric measurement of altitude (humidity in this case). Last, and most important, psychological instruments, like those of physics, can be pressed into unusual and less appropriate service by capitalizing on some minor aspect or feature of the instruments.

Several intelligence tests, including the Stanford-Binet and the Wechsler batteries, have sub-tests of vocabulary based, rightly or wrongly, on the assumption that *spontaneous* acquisition of a vocabulary is a reflection of the individual's basic capacity for learning. Anything which reduces the spontaneity (e.g., gifts of storybooks, or remedial teaching of reading) will produce changes localized in the performance of this sub-test, whose scores are part of the total. The explanation of "gains in IQ" are then essays in the obvious; the inclusion of sub-tests of arithmetic, or form relations, or of picture completions, are as likely as not, irrelevant clutter, a waste of time; the result of untidy thinking and the cause of false conclusions. It is essential that investigators consider the instruments they use, in details of underlying assumptions, mechanisms, discrete parts, and scoring weights.

Statistical analyses: use of medians. The median score, or at least its close approximation, is a little easier to find *by hand* than other measures of central tendency; you can get it as long as you can count, even if you can do no arithmetic. That is just about its only advantage, and after all who does things by hand any more? It does less than justice to interval measurement and is appreciably more unreliable than the arithmetic mean to say the least (its standard error is 25 percent larger). Worse still, strong arguments have been produced in this A.I.R. team to support a hypothesis that bimodal trends in the distributions of both independent and dependent variates will be found in many of the projects being considered; and bimodal distributions can make the median a deceptive statistic, as shown in Diagrams 3 and 4.

Diagram 3

AN EXAMPLE OF NO CHANGE IN MEDIAN BUT A CHANGE IN DISTRIBUTION

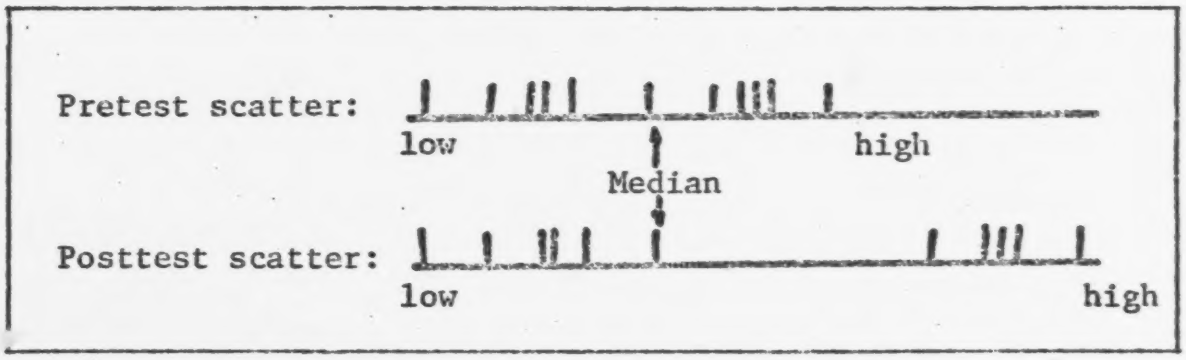
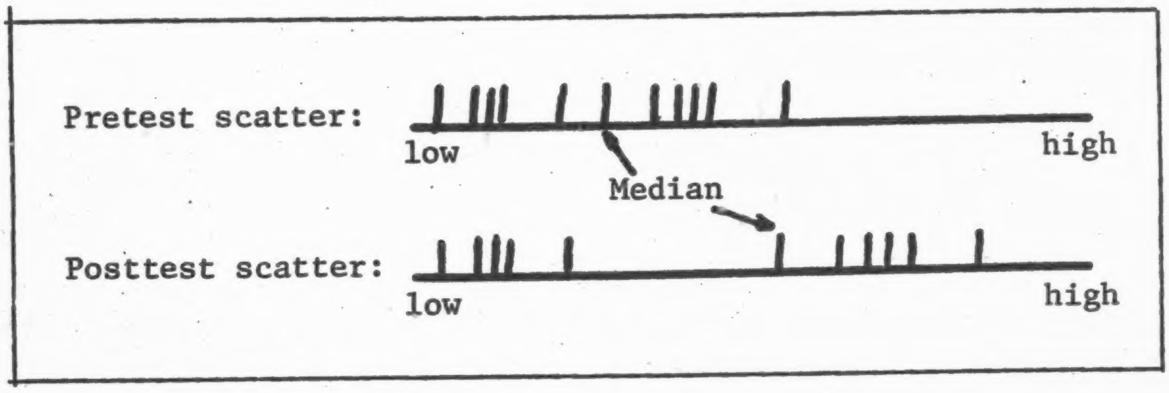


Diagram 4

AN EXAMPLE OF A CHANGE IN MEDIAN AND DISTRIBUTION



However, if gains are going to be quoted in terms of medians, there is no excuse for the failure to test the significance of such gains. All that is necessary is first to pretend that means were used and to apply the appropriate calculation of t; then divide this t by 1.253 (i.e., by $\sqrt{\pi/2}$), and refer this adjusted value to t-tables. The only incongruous element is that to calculate t, one needs the variances, which in turn need the arithmetic means. Better still, avoid the use of the median.

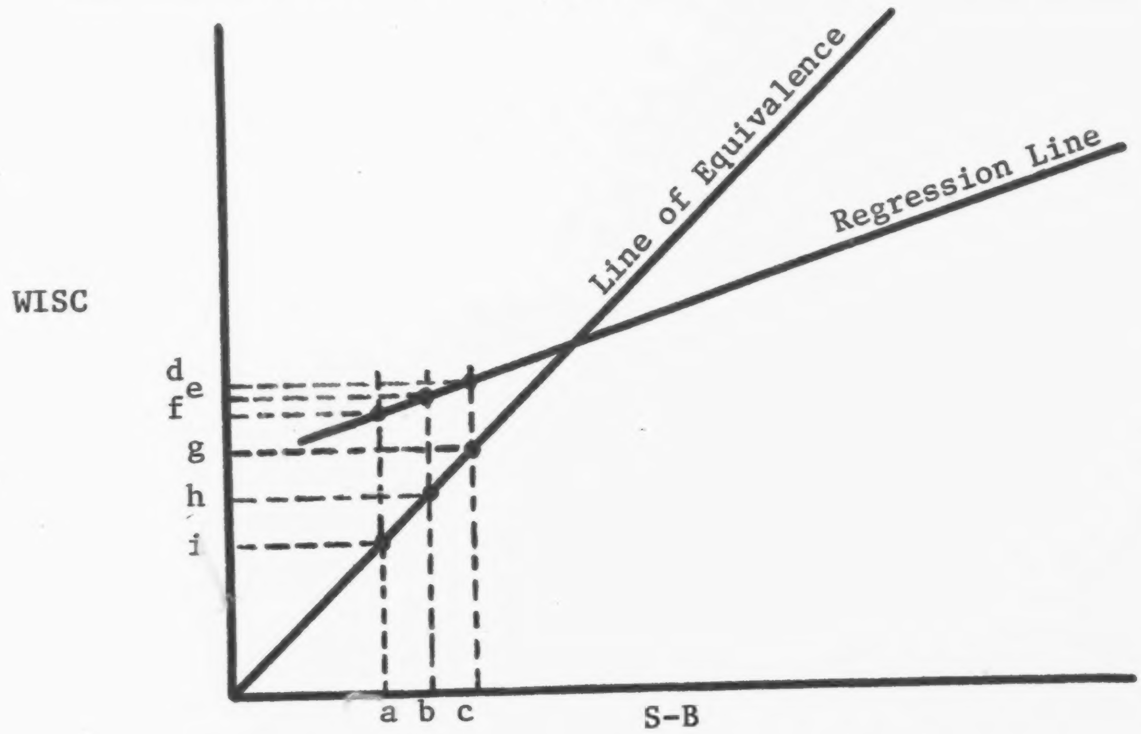
In passing, a related criticism is concerned with the use of centiles (often incorrectly referred to as "percentiles") which were sometimes found to be "averaged."

Statistical analyses: covariance analysis. No single statistical device appeared more frequently than this, for several reasons, In the first place, the commonest design involved the use of pre- and posttests of the same groups and the means were therefore correlated within groups; so covariances were to be used in any computations. Secondly, for those studies which used control samples in addition to experimental, there were often starting differences which had to be allowed for before comparing gains, and again correlations (and therefore covariances) have to be considered. Thirdly, and it is here most of all that issue can be taken, some projects, of necessity or from choice, used different tests, not only for starting and finishing, but as pretests for the two comparison groups; now gains have to be calculated after converting one or other of the reference tests, and starting differences can be detected (and allowed for) only after equivalences have been established. Even two intelligence tests like the Stanford-Binet and the Wechsler differ in their standard deviations, so that group means appearing on either side of the population mean of 100 will have distances expressed in different numbers of IQ points; an IQ of 130 on one system could easily correspond to one of 136 on the other. Here "prediction" via the regression line would appear (falsely) to be involved; the correlation coefficient is part of the regression coefficient, and covariance is part of correlation. The attempt to use "predicted" values of one test from obtained scores on another is wrong in this context; equivalence not prediction is what is needed. The argument can become quite involved and technical, and there is room here for an interpretive approach only.

The rationale upon which regressional prediction rests, is that, corresponding to every point on the continuum of the independent variate, there is a range of empirical values of the dependent variate, and one can avoid the largest errors by forcing one's prediction to be the mean of this empirical array. This array mean is a point on the regression line, and the function of the correlation coefficient is to "conservatize" the prediction. To use an analogy from baseball, one can reduce the risk of being "tagged" by always staying close to a "base," even at the cost of missed opportunities. That is why there are two regression lines, with a choice dependent upon whether one is departing continuously from A towards B, or cautiously from B towards A. One of the direct results of this conservatism is the shrinkage of the scatter of "predicted" values by comparison with that which would have obtained from direct measurement, and the shrinkage is in inverse relation to the size of the correlation coefficient. Thus for a representative sample with a real standard deviation of 15 IQ points on the WISC test, the standard deviation of scores "predicted" from the Stanford-Binet could be as low as 10 IQ points if the correlation between the two was .7. Notice then that the variance ratio (F test) for pretests (one actual and one "predicted") for two groups that were in fact identical would be 2.25. The position can be even worse if the regression lines are derived from, say, population values, and applied to samples which are not representative; even the "predicted" mean can be wrong:

Diagram 5

THE RELATIONSHIP BETWEEN EQUIVALENCE AND REGRESSION LINES



For a sample whose Stanford-Binet IQ's ranged from a to c, with a mean of b, their actual WISC values would have ranged from i to g, with a mean at h. We would have predicted a sample mean of e, on a range from f to d.

The discrepancy between the two systems stems from a difference in underlying assumptions. For predictive systems, we assume that two tests measure a common source of variance, as well as two real but not common sources, one unique to each test. In the equivalence problem, we must assume that differences between scores are the result of random error in either or both and of scaling only.

There are several alternative approaches any one of which will give less misleading results than regression; the simplest is to use the major axis of the bivariate scatter obtained empirically for a sub-sample representative of the two samples. Congruent solutions have been produced repeatedly over the past 30 years by such eminent men as Sir Cyril Burt in Britain and Frederic Lord in U.S.A., among others.

The real point, however, is this: analysis of covariance is a "real high-falutin'" process. The justification for its use, more often than not, is an attempt to compensate for errors and deficiencies in sampling, or in measurement, which were best avoided in the first place. It can add refinement to the analytic process by making fine adjustments for small discrepancies which are quantitative only. Large discrepancies may well conceal qualitative differences in which case the procedure may be inappropriate to say the least. Analysis of covariance is not such universal application as its use seems to imply. Sophistication of statistical analysis is never a compensation for deficiencies in design or measurement, and still less (if possible) for careless sampling. Where errors have been allowed to creep in, the only correct procedures or adjustments are those that make the conclusions to be drawn more conservative; and the grosser the nature of the data, the simpler should be the significance tests applied. There are some quite simple t- and chi-square tests which could take the place of some covariance analysis; leave the latter to those who are prepared to write a chapter justifying its use.

In summary, there are clear guidelines in existence for the design and evaluation of programs. Some of the most significant have been discussed here.

Two short bibliographies may be added, dealing with sampling and equating, both vital aspects of design and evaluation.

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Approaches to Cost-effectiveness Analysis in Compensatory Education

The debate about the feasibility of applying cost-effectiveness analysis to education has gone on for only a few years, but it has been a loud one. As noted earlier (in the Introduction to this report) the proponents of cost-effectiveness analysis claim that expenditures, even in education, must be justified by hard evidence rather than opinion, and that such hard evidence must be pursued relentlessly to provide the basis for cost-effectiveness analysis, which will ensure the most efficient allocation of available resources. Their opponents declare that hard evidence will never be available anyway, so the notion of applying cost-effectiveness analysis to education should be abandoned now. The proponents have yet to produce proof because few suitable models have been evolved as yet and none put into action. Their opponents can only point to the "soft" evidence now available, without proving that hard evidence is absolutely inaccessible. The debate is likely to be continued for some time. This report provides ammunition for both sides.

Cost-effectiveness analysis, properly speaking, seeks to establish the cost-effectiveness ratios of each of a series of possible alternatives, to assist in the choosing of one of them. Given a single clearly defined objective (say, over three months, the raising of reading grade-equivalents on the Metropolitan Achievement Tests by an average of ten months among a sample of 100 Harlem third-grade children reading one to two years below the national norm), we might suppose that there would be certain alternatives of instruction that could be employed. Among these would be individual tutoring by peers reading at or above the national norm, group tutoring by these same peers, individual or group tutoring by college students, or by teachers, during school or after school, teaching by computer, learning from programmed courses, and so on. The number of possible alternatives, each clearly defined, is obviously great. Theoretically, each could be costed, and a final set of ratios produced, each ratio showing the benefit (as stated in the objective) in relation to the cost of a particular treatment. Then a choice among the treatments would be feasible; normally the one costing the least would be chosen, although a complete cost-effectiveness analysis would also attempt to take the wide view, in the sense of allowing for side-effects of many kinds.

It is clear that for the kind of analysis just described considerable data are required concerning both inputs (costs and treatments) and outputs (achievements). None of these may be simplistic or overgeneralized; each must be identified and defined separately. Each bit of information must be demarcated from the next bit, lest the categorization of bits lead to faulty generalizations. In practice, in education at least, such identification, definition, and demarcation is not carried out. Costs, treatments, and achievements, none of these is analyzed in sufficient detail at present. This is not to imply that they cannot be analyzed, but it does imply that cost-effectiveness analysis is not feasible in education until these are analyzed.

If the objectives of educational programs are examined (and they in turn lead to achievements, the output), straight away the immaturity of the analytical system is revealed. The apparently clearly defined statement of an objective given above may well be inadequate for cost-effectiveness analysis, for all the fact that it is more clearly stated than most. Reading grade-equivalents are wide open to discussion as meaningful measures; different levels of the Metropolitan Achievement Tests (several of which might be used in this sample of 100 Harlem third-grade children) differ quite radically in the items they contain; an "average" of ten months could conceal a good deal about the extremes of the distribution; no basis was suggested for selecting the sample; third-grade in Harlem is not a valid indicator of anything but physical contiguity, and so on. In other words, one could not be certain about the exact nature of the output, and it would be quite wrong to attempt to generalize cost-effectiveness analysis based on this output to other apparently similar situations. Instead of an absolute analysis model, a probabilistic model has to be used, by which estimates are made of the output given variations of this kind or that. This becomes a much more complex process, with results less comprehensible to or useful for administrators.

As soon as more than short-term objectives are introduced, the definition of output becomes even more complex. Raising reading grade-equivalents during the third grade is relatively simple as an objective, but not if the long-term effects of doing so are to be taken into account. The output, over the long term, then becomes somewhat different from the objective, which was short term. The effect of raising reading age in the third may not have died out by the twelfth grade, or even beyond. Such consequences are incredibly difficult to identify, let alone quantify. (Drop-out rates, crime rates, college admission rates, marriage rates, and birth rates may all be more or less affected.) Yet they should be taken into account, since two alternative treatments may not produce the same lasting effect or long-term benefits, in which case the choice of one or the other will be influenced.

The definition of treatments poses another series of difficulties. Both intensity and quality of instruction have to be taken into account. Intensity is more easily arrived at, through attendance rates, hours of schooling, pupil/teacher ratio, and the like. Quality of instruction can be described from observation or measurement. The former provides the better account of the items used in instruction and their relationships, but offers merely a subjective assessment of quality. The latter takes note of the products of instruction but ignores the process very largely. Even these rough tools, however, show wide variations in the quality (and nature) of instruction from one classroom to another, no matter whether the teacher is held constant or not. To summarize, one cannot be sure of the treatment aspect of the input. Again, sweeping generalizations are hard to avoid, though they should be avoided.

The cost aspect of input is perhaps the one on which there is most immediate hope. The relevant prices of goods and services are what are sought. To be relevant, these prices must allow for anticipated changes in relative prices of items, although not for expected changes in the general price level (since these would be computed in any case for all alternatives); they must allow for all overhead or indirect costs; and they must omit non-essentials. All three of these requirements are hard to meet if one is examining costs of education as it is at present organized. While it may be possible to predict increases in teachers' salaries relative to increases in the costs of textbooks, it is not so easy to anticipate accurately social pressures which place teachers' aides in classrooms (or remove them) or which provide, in the more distant future, transistorized television sets for satellite transmissions in every rural community. Overhead or indirect costs may be available as some gross figure which has to be broken down to discover which of its components refer to the treatment under consideration.

The long catalog of problems just described is not peculiar to cost-effectiveness analysis in education, however. Prest and Turvey (1965), in a study supported by the Rockefeller Foundation, provide numerous examples from other fields in which similar problems are encountered. They cite as a typical case the work done during the planning of the M1 motorway (freeway) in Britain by Coburn, Beesley, and Reynolds (1960), Reynolds (1956), and Foster (1963):

> The calculation of net annual savings was classified under four heads: (i) those relating to diverted traffic; (ii) those to generated traffic; (iii) savings in non-business time; and (iv) the effects of the growth of Gross National Product. Under (i) (diverted traffic) estimates were made of the likely net savings of the traffic diverted from other routes to the M1, i.e., positive items, such as working time savings of drivers, vehicle-usage economies, petrol savings, accident reductions, etc., together with negative items in respect of additional mileages travelled on faster roads and maintenance costs of the motorway.
>
> In respect of generated traffic the argument is that the opening of the motorway would in effect reduce the "price" (in terms of congestion and inconvenience of motoring) and enable demand which had hitherto been frustrated to express itself in motorway usage. As it must be assumed that benefits per vehicle-mile to frustrated consumers are of less consequence than those to actual consumers (if not, they would not remain frustrated), they were rated as half as great as the latter in the M1 calculations.

Savings in non-business time were the third main ingredient. This calculation involves many complications, to which we shall return in a moment. The fourth component was the introduction of a trend factor, to allow for the long-term growth of Gross National Product and the effects on the demand for road travel--an obvious ingredient of any calculation, whether relating to private or public investment. The upshot of the combined calculations was that the rate of return was of the order of 10-15%.

A number of comments can be made on these calculations. First, there are the obvious statistical shortcomings which are recognized by everyone, including the authors. Second, there are a number of minor omissions, such as allowances for police and administrative costs, the benefits accruing to pedestrians and cyclists, etc., the advantages of more reliable goods deliveries. Thirdly, there are some inconsistencies in these particular calculations, in that on some occasions a long-period view seems to be taken (e.g., when calculating the savings resulting from reductions in road vehicle fleets) and on others a short-period one (e.g., in assessing the benefits of diversion of traffic from the railways). Much more important than these points are the savings due to accident reduction and to economies in travel time, where important logical and practical issues arise. On the first of these, the economic benefits of a fall in the amount of damage to vehicles and to real property, the work done by insurance companies, the work of the police and the courts are simple enough. It is the loss of production due to death or, temporarily, to accident or illness which raises complications. However, these complications are exactly the same as those raised in cost-benefit studies of health programs, and so it will be convenient to leave discussion of this general topic until we reach that heading.

This leaves us with the problem of valuing time savings; as these savings often form a very high proportion of total estimated benefits of road improvements, they are extremely important. Unfortunately, these calculations have not so far been very satisfactory.

Whatever the valuation procedure followed, it is necessary to assume that one time saving of sixty minutes is worth the

> same as sixty savings each lasting one minute, since estimates of the value of time savings of different lengths are unobtainable. On the one hand, it is clear that some short-time savings are valueless, since nothing can be done in the time saved. On the other hand, however, there are cases where the extra time makes possible some activity which would otherwise be precluded, as, for instance, when arriving a little earlier at a theatre means that one does not have to wait until the interval to gain one's seat. Similarly, the value of an hour gained may depend partly upon when it is gained. Faute de mieux, such variations have to be ignored and an average treated as meaningful.

Prest and Turvey conclude by saying, with regard to this case:

> So at the very least, one can say that there are major unknowns which may or may not prove tractable to further analysis.

Many studies of cost-effectiveness in education have taken a much broader view, in an attempt perhaps to avoid the limitations imposed by more rigorous definition. For instance, there have been those which have concentrated on the costs of higher education, including the loss in income suffered by students while studying, and the benefits to be accrued in the form of higher incomes after graduation. Becker (1964), Hansen (1963), and Hunt (1963) deal with such comparisons. Hunt applied multiple-regression techniques in an attempt to detect relationships more accurately, but Prest and Turvey (1965) remark that the confidence intervals of his estimates of the effects of various variables were very wide, and it was made abundantly clear that correction for other determinants of income was very necessary. Blaug (1965) considered such difficulties in some detail.

Other studies, such as that by Denison (1964), have examined the effect of education upon the national product. Denison calculated that education in combination with a factor called "advance of knowledge" accounted for about two-fifths of the growth of real income in the United States from 1929 to 1957. In these studies, education is viewed as a process of investment in people. The payoffs are discussed by Weisbrod (1962) who sees them in broad terms of increasing production possibilities, reducing costs (thereby making resources available), and contributing to the general welfare. Spiegelman et al. (1968) have added another: increase in benefits which increase an individual's enjoyment of life.

The Spiegelman et al. study (1968) examined not only the increase in personal income due to graduating from high school and going to college, but also intergeneration benefits (those accruing to the offspring of the generation currently being educated), and the reduction of juvenile crime. These three selected outputs were related to inputs paid for by ESEA Title I funds; they were not intended to be comprehensive and others could certainly have been selected. The study developed a benefit/cost model along fairly generalized lines, analyzed the benefits to be expected (in terms of direct increase in attainment, probability of high school graduation, probability of going to college, intergeneration effects, and probability of being arrested for committing a juvenile crime), and computed the returns to education. Equations were derived which attempted to take into account a number of considerations of the kind discussed earlier in this section.

The Technomics (1967) report contained much of value in the setting up of cost-effectiveness analysis in education. The Technomics team discussed very succinctly the data required to accomplish such analysis, and made every effort to secure the data itself, so that their model could be used. The team investigated the ESEA Title I projects of 12 cities to discover what costs, treatments, and achievements there were. The conclusions reported were that "it *is* feasible to apply a *suitably modified version* of cost/effectiveness technology to matters of educational expenditure; furthermore, we can describe the necessary modifications in considerable detail. We can also specify an easily understood tool for program planning and budgeting that will be *immediately usable* at the local level." This optimistic viewpoint was indeed supported by a model, which employed two special techniques. The first, cluster analysis, was claimed to allow the user to associate benefits (the achievements) and costs with the characteristics of a learning environment. The second, a matrix, was employed to define and clarify the structure of the "educational possibility space."

The Technomics team was certainly not guilty of a facile approach, but its optimism may be questioned. The development of the model involved the setting up of a large number of categories, into which similar but sometimes differing items were compressed. To do so required the making of various untested assumptions. When it came to applying data to the model, further compressions were necessary on account of the nature of the data. In spite of the fact that Technomics was able to survey 22 projects in 12 cities, which accounted for expenditures of nearly half a billion dollars, the number of cases proved too limited to provide adequate information on many variables. Low information variables were therefore eliminated from the treatment, at unestimated costs to the model as a whole. This much was admitted in the report. A list of project variables was derived, the scope and limitations of which are best depicted by the list itself:

A1. Focus on child (identifying and meeting special needs)
A2. Redefinition of learning
A3. In-service training
A4. Administrative staff support
A5. Teaching staff support
A6. Cooperation of teaching and administrative staff
A7. Services
A8. Methodology
A9. Attention to the child in class

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A10. Freedom to experiment with teaching methods
A11. Parental involvement
A12. Community involvement
A13. Project planning
A14. Class size
A15. Classroom management techniques
A16. Breadth of program (number of levels of instruction)
A17. Methods of selection of pupils for the project
A18. Racial mix of project group
A19. Racial mix of school

Few of these variables stand up well to close examination. Most require subjective definition subjectively applied, thus adding to the qualitative element of the base for an apparently quantitative analysis, and thereby increasing the chances for error. The benefit variables suffer from the same problems.

Cluster analysis, one of the techniques mentioned earlier, again depends on compressions. To group similar variables, judgments have to be made about their similarity. As the Technomics report indicated, cluster analysis is used to reduce the number of variables that must be handled in the statistical analysis while minimizing the loss of information entailed by the reduction. Probabilities once again enter the model, rendering the final conclusions less trustworthy.

The views of the Technomics team are the more remarkable in the light of the comments in the report concerning the difficulties experienced in data collection, difficulties equally experienced by the AIR team two years later (as described under Methods and Procedures). To quote one relevant portion from the Technomics report:

> Most damaging of all, however, was the use of inappropriate evaluation procedures. Related measures were commonly tested as though they were independent; the possibility of multiple causation was ignored in testing gains; control groups were almost never used to provide either baseline or comparative data. Projects were allocated to schools on bases absolutely unrelated to evaluation design needs.

The chances of producing a useful cost-effectiveness analysis based on data from such basic errors, even with a fine model, seem remote. The predictions made by Technomics to the effect that the data would soon improve as planning (particularly under Title I) matured, have not been borne out by AIR experience. Projects still do not have clear and workable objectives, benefits to be derived appropriately stated, and achievements accurately measured.

Technomics made many good suggestions, however, for improving systems and evaluations so that better data would be available in the future, to apply to the Technomics model. (Similar suggestions appear under Guidelines for Program Design and Evaluation in this report.) Most of the changes related to evaluation and accounting procedures, rather than to the description of the nature and quality of treatments.

In concluding this discussion of the Technomics study, it must be emphasized that the Technomics report repeatedly discussed the difficulties. What is puzzling is the belief of the team that a viable tool had been developed. Perhaps evidence of its viability will be forthcoming soon from some project which follows the Technomics recommendations and uses the program planning and budgeting matrix, the planning vectogram, and cluster analysis.

The most critical difficulty in the way of cost-effectiveness analysis in compensatory education at present is one scarcely mentioned in the Technomics study, but one which became very clear during this survey. In order to be able to select from alternative treatments, it is essential that each of the treatments carry some guarantee that it will result in the anticipated output (benefit). Seven different treatments at differing costs, may be presented as means of reaching a particular goal, say raising by one year the reading grade-equivalents of a certain group of pupils. All seven must have been shown to have been successful with similar pupils elsewhere. Evidence that alternative successful treatments exist is not available at present among compensatory education programs in the United States. Even if the programs with measured benefits of cognitive achievement which were not studied during this survey were included, it is very doubtful whether there would be enough examples available to begin cost-effectiveness analysis. Comparisons between treatments which have yielded differing amounts of the same type of benefit might just be possible, but would be fraught with hazards on account of the non-additive quality of some benefits.

Thus the first step towards cost-effectiveness analysis must be through improving compensatory programs until the treatments can be said to be successful. At present the cost-effectiveness choices lie almost entirely among the more or less unsuccessful, which makes nonsense of cost-effectiveness analysis at all.

The view taken in this A.I.R. study is that cost-effectiveness analysis in education, in the sense of providing cost-benefit ratios to assist in choices among alternatives, cannot yet be undertaken. The Technomics model cannot be applied successfully, nor can the much more theoretical one of Abt (1967), until improvements are made in programs of compensatory education, and in the schools' systems of recording and reporting inputs and outputs. The improvements in the programs may be assisted by this study, in that the existing successes are advertised more widely.

A move towards improving the schools' systems does appear to be feasible, and the current (1968) GE-Tempo study may be assisting it. The A.I.R. survey has shown repeatedly that the majority of programs have been set up as demonstrations, not experiments, including a variety of components which local educators confidently expected would improve the education of the children involved. A few programs were specifically established as research projects to test the teaching efficiency of groups of components. Few school systems or research centers have records of the details of components of programs, their costs, or the results. The aim of authorities involved in compensatory education has been to teach rather than to keep such records, and their attitude tends to be that the data necessary for cost-effectiveness studies probably do not exist, but if they do, many man/days would be required to extract them.

Selected programs could be assisted to establish and maintain the types of records necessary for cost-effectiveness studies. Experience in this survey has shown that there are some programs which would tolerate such assistance, and others which would welcome it. The immediate purpose would be to build into these programs tests and observations, records and accounts in such a way that some analysis would become feasible. Since the programs themselves would have access to the raw data from the "dials and gauges" installed, it would be generally to their advantage to cooperate. Those who assisted the programs to make these changes would be consultants on the structuring of recording and evaluation, rather than being evaluators. Their services would assist in the modification of programs in ways which would not hinder operations, but would improve evaluation.

Since such a scheme would be accepted only by authorities who wished it, it could not be construed as a plan to introduce more governmental control. To A.I.R., it represents the best chance of moving towards cost-effectiveness studies in this field.

To conclude, some further statements on cost-effectiveness should be quoted from Prest and Turvey:

> An important advantage of a cost-benefit study is that it forces those responsible to quantify costs and benefits as far as possible rather than rest content with vague qualitative judgments or personal hunches. This is obviously a good thing in itself; some information is always better than none. Furthermore, quantification and evaluation of benefits, however rough, does give some sort of clue to the charges which consumers are willing to pay. It may well be a salutary check on the biases likely to creep into estimated costs and benefits by enthusiastic advocates of particular projects, that, wherever technically feasible, some charges should be imposed. The discipline of the marketplace is so easily and so readily forgotten in these situations that some empirical evidence about benefit projections is highly necessary. Insistence on some charging process may therefore be a sensible antidote to wilder excesses of particular lobbies. We must remember, however,

that the extent to which the authorities impose these charges brings in distributional as well as efficiency calculations.

Even if cost-benefit analysis cannot give the right answers, it can sometimes play the purely negative role of screening projects and rejecting those answers which are obviously less promising ...

The case for using cost-benefit analysis is strengthened, not weakened, if its limitations are openly recognized and indeed emphasized. ... It is no good expecting those fields in which benefits are widely diffused, and in which there are manifest divergences between accounting and economic costs or benefits, to be as cultivable as others. Nor is it realistic to expect that comparisons between projects in entirely different branches of economic activity are likely to be as meaningful or fruitful as those between projects in the same branch. The technique is more useful in the public-utility area than in the social-services area of government. Comparisons between, say, different road projects are more helpful than those between, say, road and water projects; and both these are likely to be more helpful than applications in the fields of education, health, research, and so on.

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Organization of Bibliography

General Sources. Publications in the General Sources section fall into one or more of the following categories:

1. Bibliography, book, paper, journal article, or periodical on the "disadvantaged," "culturally deprived," "socio-culturally disadvantaged," and related topics;

2. Evaluation report of a single project conducted in several localities within a state;

3. Evaluation report of a single project conducted in several states;

4. Collection of evaluation reports on several projects conducted within a state;

5. Collection of evaluation reports on several projects conducted in several states.

Sources for Programs Studied but not Described. Publications in this section fall into one or more of the following categories:

1. Collection of evaluation reports of several projects conducted within a city, or school, or school district (see subheading, "Collection of Evaluation Reports");

2. Evaluation report and/or related materials pertaining to a single project conducted within a city, or school, or school district (see subheading, "Specific Projects").*

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* This heading does not appear when a set of publications contains only reports on specific projects, and no collections of reports (e.g., Arizona [p. 70] vis-a-vis California [pp. 71 -79]).

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(1) Materials re EIP administration and evaluation.
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(1) Materials re EIP administration and evaluation.

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APPENDIX A

FULL TITLE OF PROJECT:

SHORT TITLE (IF ANY):

LOCATION DATA:

PROJECT DIRECTOR(S): 1. TEL: 1.
2. 2.

ADDRESSES: 1.
2.

FUNDS SOURCE: FUNDS TOTAL:

AGENCY MONITOR/SUPT.:

RELATED PUBLICATIONS:

REMARKS:

GRADE LEVEL (Circle): -2 -1 K 1 2 3 4 5 6 7 8 9 10 11 12

SUBJECTS: N:

BEHAVIOR(S) MEASURED:

MEASURE(S) EMPLOYED:

BENEFITS CLAIMED:

FOLLOW-UP: DESIGN RATING: 1 2 3 4 5 DATE OF TERMINATION:

APPENDIX B

March 29, 1968 G-52

DESIGN RATING ITEMS

1. The population is described . . . .
2. The sampling procedures are fully described . . . .
3. Objectives are clearly specified in detail. . . . .
4. The criteria are clearly specified. . . . .
5. A list is given of the variables considered . . . .
6. Measuring instruments used are directly related to the objectives . . . .
7. Measuring instruments used are either well-known or fully described . . .
8. Data are recorded in tables . . . .
9. The types of statistical analyses employed are described. . . . .
10. Significance (P) levels are quoted. . . . .
11. Follow-up data are quoted . . . .

Rating	1	2	3	4	5
Points	0-2	3-4	5-6	7-8	9-11

APPENDIX C

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